WORKS

OF

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LIVES OF PHILOSOPHERS

OF THE TIME OF GEORGE III.

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PREFACE.

THE reign of George III. may in some important respects be justly regarded as the Augustan age of modern history. The greatest statesmen, the most consummate captains, the most finished orators, the first historians, all flourished during this period. For excellence in these departments it was unsurpassed in former times, nor had it even any rivals, if we except the warriors of Louis XIV.'s day, one or two statesmen, and Bolingbroke and Massillon as orators. But its glories were not confined to those great departments of human genius. Though it could show no poet like Dante, Milton, Tasso, or Dryden; no dramatist like Shakspeare or Corneille; no philosopher to equal Bacon, Newton, or Locke,—it nevertheless in some branches, and these not the least important of natural science, very far surpassed the achievements of former days, while of political science, the most important of all, it first laid the foundations, and then reared the superstructure. The science of chemistry almost entirely, of political economy entirely, were the growth of this remarkable era; while even in the pure mathematics a progress was made which nearly changed its aspect since the days of Leibnitz and Newton. The names of Black, Watt, Cavendish, Priestley, Lavoisier, Davy, may justly be placed far above the Boyles, the Stahls, the Hales, the Hookes of former times; while Euler, Clairaut, D'Alembert, Lagrange, La Place, must be ranked

as analysts next after Newton himself, and above Descartes, Leibnitz, or the Bernouillis; and in economical science, Hume, Smith, and Quesnai really had no parallel, hardly any forerunner. It would also be vain to deny great poetical and dramatic genius to Goldsmith, Voltaire, Alfieri, Monti, and the German school, how inferior soever to the older masters of song.

But, above all, it must not be forgotten, that in our times the mighty revolution which has been effected in public affairs, and has placed the rights of the people throughout the civilized world upon a new and a firm foundation, was brought about, immediately indeed by the efforts of statesmen, but prepared, and remotely caused, by the labours of philosophers and men of letters. The diffusion of knowledge among the community at large is the work of our own age, and it has made all the conquests of science both in recent and in older times of incalculably greater value, of incomparably higher importance to the interests of mankind, than they were while scientific study was confined within the narrow circles of the wealthy and the learned.

Having, therefore, on retiring from office, more time left for literary pursuits than professional and judicial duties had before allowed me, I was not minded to waste, indolent and inactive, or enslaved by lower occupations, that excellent leisure:—"Non fuit consilium socordiâ atque desidiâ bonum otium conterere; neque vero agrum colendo, aut venando, servilibus officiis intentum, ætatem agere. Statutum res gestas populi nostri carptim, ut quæque memoriâ digna videbantur, perscribere; eo magis quod mihi a spe, metû, partibus reipublicæ, animus liber erat." * For I conceived that as portrait-painting is true historical painting

* Sall., Cat., cap. iv.

in one sense, so the lives of eminent men, freely written, are truly the history of their times; and that no more authentic account of any age, its transactions, the springs which impelled men's conduct, and the merits which different actors in its scenes possessed, can be obtained than by studying the biography of the personages who mainly guided affairs, and examining their characters, which by their influence they impressed upon the times they flourished in. Such a work had moreover this advantage, that beside preserving the memory of past events, and the likeness of men who had passed from the stage, it afforded frequent opportunities of inculcating the sound principles of an enlightened and virtuous policy, of illustrating their tendency to promote human happiness, of exhibiting their power to exalt the genuine glory as well of individuals as of nations.

Though I could entertain little doubt that this plan was expedient, no one could more doubt than I did the capacity brought to its execution, or feel more distrustful of the pen held by a hand which had so long been lifted up only in the contentions of the Senate and the Forum. My only confidence was in the spirit of fairness and of truth with which I entered on the performance of the task; and I now acknowledge with respectful gratitude the favour which the work has hitherto, so far above its deserts, experienced from the public, both at home, in spite of party opposition, and abroad, where no such unworthy influence could have place. It is fit that I also express my equal satisfaction at the testimony which has been borne to its strict impartiality by those whose opinions, and the opinions of whose political associates, differed the most widely from my own. That in composing the work I never made any sacrifice of those principles which have ever guided my public conduct, is certain; that I never concealed them in

the course of the book is equally true; nay, this has been made a charge against it, as if I was at liberty to write the history of my own times, nay, of transactions in many of which I had borne a forward part, and not show what my own sentiments had been on those very affairs. But if my opinions were not sacrificed to the fear that I might offend the living by speaking plainly of the dead, so neither were truth and justice ever sacrificed to those opinions.

The Statesmen of George the Third's age having thus formed the subject of the volumes first published, I then gave a more full and elaborate view of the Learned Men who flourished in the same period. In my opinion, these, the teachers of the age, covered it with still greater glory than it drew from the Statesmen and the Warriors who ruled its affairs. It was necessary to enter much more into detail here than in the other branch of the work, because a mere general description of scientific or of literary merit is of exceedingly little value, conveying no distinct or precise idea of the subject sought to be explained. It appeared the more necessary to discuss these matters minutely, because upon some of them much prejudice prevailed, and no attempt had hitherto been made to examine them completely, or even impartially. Of this a remarkable example is afforded by the want of anything that deserves the name of a Life of Voltaire, and by the great prejudices, both favourable and unfavourable to him, which, among different classes, exist on the subject. But it must also be observed that Dr. Black's discoveries have been far from attaining the reputation which they so well deserve as the foundation of modern chemistry; and justice to this illustrious philosopher required that the consequences arising from his modesty and his great indifference to fame should be counteracted by a full history of his scientific labours, comparing the state of the science as he found it with that in which

he left it.—My own personal acquaintance with some of the great men whose history I ventured to write, enabled me to throw additional light upon it; and respecting one, whom of course I could not have known, Mr. Hume, I obtained information from good sources through the kindness of friends. The materials of his life are, however, chiefly to be sought in his writings, and especially in his letters. The same remark is applicable to the Life of Voltaire. Those who have written it, like the Marquis de Condorcet, without ever referring to the fourteen large volumes (containing nine thousand closely-printed pages) of his Correspondence, might just as well have undertaken to give a life of Rousseau without consulting his 'Confessions,' or of Hume, without reading his 'Autobiography.'—I have, besides, had access to valuable original documents both of Voltaire, Robertson, and Cavendish; to some respecting Watt and Simson.

Scientific and literary history, the record of the progress of science and of letters, and which is most usefully given in the lives of their cultivators, serves two purposes; the one historical and critical, the other didactic. It is of great importance to trace the progress of mankind in the advancement of knowledge, and its diffusion; to show by what steps improvements have been made and applied; to estimate the relative merits of those whose claims upon our gratitude are the most unquestionable; and to ascertain the position in which their labours have left the subjects of those labours, with the aspect and extent of the region that yet remains unexplored. But, it is hardly a less valuable service of such works that they promote the knowledge of the subject matter, both by exciting the desire of it, and by facilitating its acquisition. The history of a philosopher's life, that is, of his labours, the tracing of those steps by which he advanced beyond his predecessors, the com-

parison of the state of the science as he found it, with that in which he left it, tends mightily to interest the reader, to draw him towards the same inquiries, and to fix his views more closely upon the details of the subject, if it has already somewhat occupied his mind. In like manner, the recording and the description of literary labours and merits, in connexion with the historians, poets, and orators themselves, has a powerful effect in making the reader familiar with the subject, while it cultivates and refines his taste.

Under the head of Philosophers, it is unnecessary to observe upon any of the lives except those of Adam Smith, D'Alembert, and Simson, unless to note, that those of Black and Lavoisier give a full statement of the relative merits of these great men, and of the conduct of the latter, both with regard to Black and Priestley. But as many persons entertain a prejudice against the pretensions, or it may be, against the practical conclusions of the Political Economists, they may be apprised that the subjects on which the great and well-established fame of Adam Smith is founded, are here treated without any of the exaggerations wherewith speculative economists have been charged, and that the Life, and the Analysis of his great work were written long before the question respecting Free Trade and the repeal of the Corn Laws had assumed a practical form. Whatever touches that question, was composed as a treatise upon a subject of science only, with the desire to discover and to expound the truth, and without any view to the interests of any party,—the author, though he entirely approved the repeal, yet neither agreeing with those who hoped, nor with those who feared, so much from its consequences.

The Lives of Simson and D'Alembert, are designed not only to give the history of these eminent men—the restoration of the ancient geometry by the former, and the improve-

ment of the modern analysis by the latter—but also to convey a competent knowledge of those great methods; while in both lives, especially that of D'Alembert, there is further presented a strong recommendation of mathematical pursuits, by showing the gratification which they are fitted to bestow. Great as is the value of Montucla's History, in the light of a didactic work, many readers have lamented not more that he left it unfinished, and the latter half very unsatisfactorily edited, than that he did not enter more fully into the detailed statement of the subject, in several of the earlier portions.

By such historical and critical works, then, the desire and the acquisition of science is promoted; and surely no more important duty can be performed, than that of affording both the excitement and the gratification, in however moderate a degree. They who are wholly incapable of advancing science themselves, may help others to the knowledge of what the great masters have done; and they may do this best by not disdaining the office of elementary explanation and discussion. Two thousand years ago, the wisest of the ancients was said to have brought philosophy down from heaven to earth; certainly, he chiefly valued himself on his constant efforts to stir up in men's minds the desire of knowledge.* What he found necessary with regard to the nature of the subject, we in our day may perceive to be equally necessary because of the clouds in which great men, almost unavoidably, involve their scientific researches. The mathematical writings of Newton and his immediate successors require to be made plain, and also to be illustrated by comparative discussion, in order both to show exactly what they accomplished, and to excite an intelligent curiosity respecting their labours. This has been

* Cic. Acad., Qu. i. 4, Tusc. v. 4.

the object both of the Life of D'Alembert, and of the Analytical View of the Principia.*

The course of this work kept me for the most part, at a distance from questions touching political affairs, or the constitution and progress of society, but not always. The reader will find that no opportunity has been left unimproved, as far as I was capable of seizing it with any effect, for inculcating or illustrating the great doctrines of peace, freedom, and religious liberty. The observations on historical composition in the Life of Robertson, I especially consider as pointing to an improvement in that department of letters, highly important to the best interests of mankind, as well as to the character of historians.

But although I had no political animosities to encounter, I feared my historical statements and my commentaries on some lives, as those of Voltaire, Rousseau, and Hume, might find enemies among the two great parties whose principles came in question. The Free-thinkers might object to the blame which I ventured to pronounce upon their favourite authors; the friends of the Church might take exception to the praises occasionally bestowed. It may, however, be expected from the justice of both these conflicting bodies, that they will read with attention and with calmness before they condemn. From the former class I could expect no favour beyond what every one has a

* The Analytical View, first published in 1839, omitted the Second and part of the Third Book. The whole is now nearly completed. The object was to enable persons having little mathematical knowledge, beyond elementary geometry and algebra, to follow the demonstrations of the fundamental propositions, and to understand by what kind of reasoning the others are proved. That it was successful in this respect, there were undoubted proofs; but the discussions with which the investigations were interspersed had also a very material effect.

right to claim from avowed adversaries; a fair hearing was all I desired. To the latter a few words might be addressed in the spirit of respectful kindness, as to those with whom I generally agree.

Whoever feels disposed to treat as impious any writer that has the misfortune not to be among the great body of believers, like the celebrated men above named, should bear in mind that the author of these pages, while he does justice to their great literary merits, has himself published, whether anonymously or under his own name, nearly as much in defence of religion as they did against it; and if, with powers so infinitely below theirs, he may hope to have obtained some little success, and done some small service to the cause of truth, he can only ascribe this fortune to the intrinsic merits of that cause which he has ever supported.* He ventures thus to hope that no one will suspect him of being the less a friend to religion, merely because he has not permitted his sincere belief to make him blind regarding the literary merit of men whose opinions are opposed to his own. His censures of all indecorous, all unfair, all ribald or declamatory attacks, however set off by wit or graced by eloquence, he has never, on any occasion, been slow to pronounce.

BROUGHAM, *3d January*, 1855.

* It has given me a most heartfelt satisfaction to receive many communications from persons both at home and abroad, which intimated their having been converted from irreligious opinions by the 'Commentaries and Illustrations of Paley,' published in 1835 and 1838.—It must be noted that the passage of the present work in which Dr. Lardner is mentioned as an orthodox writer, refers to the great question between Christians and Infidels. He was an Unitarian, undoubtedly; but his defence of Revelation forms really the groundwork of Dr. Paley's 'Evidences.'

One Volume of the original publication was dedicated to the late Mr. Justice Williams, "as a small memorial of ancient friendship;" the other to Prince Albert, "in token of respect for his encouragement of letters and the arts." The French translation of the Lives of Voltaire and Rousseau, was inscribed to Lord Howden, "as a feeble testimony of old and constant friendship."

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PHILOSOPHERS

OF

THE TIME OF GEORGE III.

BLACK.

THE physical sciences have few more illustrious names to boast than that of Joseph Black. With all the habits and the disciplined faculties of a true philosopher, with the temper as well as the capacity of a sage, he possessed that happy union of strong but disciplined imagination, with powers of close undivided attention, and ample resources of reasoning, which forms original genius in scientific pursuits; and, as all these qualities may be combined in an individual without his happening to signalise his investigations of nature by any discovery, we must add that his life was crowned with the good fortune of opening to mankind new paths in which both himself and his followers successfully trod, enlarging to an incalculable extent the bounds of human knowledge. The modesty of his nature making him averse to publish his speculations, and the genuine devotion to the investigation of truth, for its own sake, rendering him most open in his communications with all who were engaged in the same pursuits, his incontestable claim to be regarded as the founder of modern chemistry has been oftentimes overlooked; and, while some have endeavoured more or less obscurely to mingle themselves with his discoveries, others have thought it becoming to post-date the new

system, that it might seem the produce of a somewhat later age. The interests of truth and justice therefore require that we should minutely examine the facts of the case; and, happily, the evidence is so clear that it only requires an attentive consideration to remove all doubt from the subject. I feel it a duty imperatively cast upon me to undertake a task from which, did I not regard it as less difficult than sacred, I might shrink. But I had the great happiness of being taught by himself, having attended one of the last courses of lectures which he delivered; and the knowledge thus gained cannot be turned to a better use than in recording the glory and in vindicating the fame of my illustrious master.

The story of a philosopher's life is soon told. Black was born, in 1721, at Bordeaux, where his father, a native of Belfast, was settled as a merchant: he was, however, a Scotchman, and his wife too was of a Scottish family, that of Gordon of Hillhead, in Aberdeenshire, settled like Mr. Black at Bordeaux. The latter was a person of extraordinary virtues, and a most amiable disposition. The celebrated Montesquieu honoured him with his especial regard; and his son preserved, as titles of honour in his family, the many letters of the President to his parent. In one of them he laments the intended removal of the Black family as a thing he could not reconcile himself to, for his greatest pleasure was seeing them often, and living himself in their society. Though Mr. Black sent his son, at the age of twelve, for some years to a school in Ireland, he was removed to the College of Glasgow in the year 1746, and ever after lived in that which was, properly speaking, his native country. At that college he studied under the celebrated Cullen, then Professor of Anatomy and Lecturer on Chemistry; and, having removed in 1750 to Edinburgh for the benefit of that famous medical school, he took his degree there in 1754. In 1756 he was appointed to succeed Dr.

Cullen in the chair of anatomy and chemistry at Glasgow, and he continued to teach there for ten years, when he was appointed to the chemistry professorship at Edinburgh. He then lectured for thirty years to numerous classes, and retiring in 1796 lived till 1799, and died on the 26th of November in that year. His health never was robust; it was indeed precarious at all times from a weakness in the bronchia and chest, but he prolonged life by a system of the strictest abstinence, frequently subsisting for days together on water-gruel and diluted milk. He never was married; but he cherished with unvarying affection his near relatives, who well deserved his care. His favourite niece, Miss Burnet, a person of great sense and amiable temper, was married to his friend and second cousin, Professor Ferguson, the historian and moral philosopher. Dr. Black lived in a select circle of friends, the most illustrious men of the times in science and in letters, Watt, Hutton, Hume, Robertson, Smith, and afterwards with the succeeding generation of Scottish worthies, Robison, Playfair, Stewart. Delighting to commune, to speculate, and to investigate with them, he was careless of the fame which however he could not but be sensible his labours must achieve. He was extremely averse to publication, contemning the impatience with which so many men of science hurry to the press, often while their speculations are crude, and their inquiries not finished. Nor could the reason often urged in defence of this find much favour with one who seemed never to regard the being anticipated by his fellow-labourers as any very serious evil, so the progress of science was secured. Except two papers, one in the 'London Philosophical Transactions' for 1775 on the freezing of boiled water; the other, in the second volume of the 'Edinburgh Transactions,' on the Iceland hot springs; he never published any work after that of which we are now to speak, in 1755, and which, but for the accidental occasion that gave rise to it,

would possibly, like his other original speculations, never have been given by himself to the press.

Upon taking his degree at Edinburgh College he wrote and published a Latin Thesis, after the manner of that as well as the foreign universities. The subject was 'Magnesia, and the Acid produced by Food in the Stomach' (*De Acido e Cibis orto; et de Magnesia*), and it contained the outline of his discoveries already made. Having sent some copies of this Thesis to his father at Bordeaux, one was given to Montesquieu, who at once saw the vast importance of the truths which it unfolded. He called a few days after and said to Mr. Black, "I rejoice with you, my very good friend: your son will be the honour of your name and of your family." But though the discoveries were sketched distinctly enough in this writing, they were only given at large the following year in his celebrated work 'Experiments on Magnesia, Quicklime, and other Alkaline Substances,' incontestably the most beautiful example of strict inductive investigation since the 'Optics' of Sir Isaac Newton. His fervent admiration of that masterly work was indicated by his giving it to Professor Robison, then a student, and desiring him to "make it the model of all his studies," recommending him at the same time a careful study of the mathematics. It appears that this important inquiry concerning the alkaline earths, the results of which were destined to change the face of chemical science, was suggested by the attempts then making to find a solvent for the stone. I distinctly recollect Dr. Black, in his lectures, prefacing the admirable and most interesting account which he gave of his discoveries, with the statement that the hopes of finding a solvent which should not, like the caustic alkalies, destroy the substance of the bladder in melting the stone, first led him to this investigation. Professor Robison has given a note from his memorandum-book indicating that he had at first fallen into the notion of alkalies, when treated with quicklime,

deriving from it their caustic quality; the common belief (which gave rise to the term caustic) being that lime obtained from the fire the quality of growing extremely hot, even to ignition when united with water. But experiment soon corrected this idea; for, having exposed the caustic or quicklime to the air till it became mild, he says, "Nothing escapes (meaning no fire or heat); the cup rises considerably by absorbing air." Another observation on the comparative loss of weight sustained by chalk when calcined (in the fire), and when dissolved in an acid, is followed by the account of a medical case, which the Professor knew to have occurred in 1752. A third note follows, and proves him to have now become possessed of the true theory of causticity, namely, the expulsion of air, and of mildness, namely, its absorption. The discovery was therefore made as early as 1752—it was published generally in 1754—it was given in its fullest details in 1755. At this time M. Lavoisier was a boy at school —nine years old when the discovery was made—eleven when it was published—twelve when it was as fully given to the world as its author ever delivered it. No possibility therefore existed of that great man finding out when he composed his great work that it was a discovery of his own, as he did not scruple to describe oxygen, though Dr. Priestley had first communicated it to him in the year 1774; or that Black and he discovered it about the same time, as he was in the habit of stating with respect to other gases, with a convenient degree of ambiguity just sufficient for self-defence, should he be charged with unfair appropriation. Who that reflects on the noble part which this great philosopher acted, both in his life and in his death, can avoid lamenting that he did not rest satisfied with the fame really his due, of applying the discoveries in which he had no kind of share, to the investigation of scientific truths, as entirely the result of his extraordinary faculty of generalization, and genius for philosophical

research, as those discoveries, the materials of his induction, were the undivided property of others!

The capital discovery of Black, thus early made, and to any share in which no one has ever pretended, was that the causticity, as it was formerly termed upon a false theory, of the alkalis and alkaline earths, was owing to the loss of a substance with which they had been combined, and that their reunion with this substance again rendered them mild. But the nature of this substance was likewise ascertained by him, and its detection forms by far the most important part of the discovery, for it laid the foundation of chemical science. He found that it was a permanently elastic fluid, like air in some of its mechanical qualities, those of being transparent or invisible, and incondensable, but differing entirely from the air of our atmosphere in its chemical properties. It was separated from alkaline substances by heat, and by the application of acids, which, having a stronger elective affinity for them, caused it to be precipitated, or to escape in the aëriform state; it was heavier than common air, and it gave a slight acidulous flavour to water on being absorbed by it; hence the inference that it was an acid itself. A short time afterwards (in 1757) he discovered that this peculiar air is the same with that produced by the fermentation of vegetable substances. This he ascertained by the simple experiment of partially emptying in a brewer's vat, where the fermenting process was going on, the contents of a phial filled with lime-water. On shaking the liquid that remained with the air that had entered, he found it become turbid, from the lime having entered into union with the air, and become chalk. The same day he discovered by an experiment, equally simple and equally decisive, that the air which comes from burning charcoal is of the same kind. He fixed a piece of charcoal in the broad end of a bellows nozzle, unscrewed; and putting that in the fire, he inserted the other end in a vessel filled with lime-water.

The air that was driven through the liquid again precipitated the lime in the form of chalk. Finally, he ascertained by breathing through a syphon filled with lime-water, and finding the lime again precipitated, that animals, by breathing, evolve air of this description.

The great step was now made, therefore, that the air of the atmosphere is not the only permanently elastic body, but that others exist, having perfectly different qualities from the atmospheric air, and capable of losing their elasticity by entering into chemical union with solid or with liquid substances, from which being afterwards separated, they regain the elastic or aëriform state. He gave to this body the name of *fixed air*, to denote only that it was found fixed in bodies, as well as elastic and separate. He used the term "air" only to denote its mechanical resemblance to the atmospheric air, and not at all to imply that it was of the same nature. No one ever could confound the two substances together; and accordingly M. Morveau, in explaining some years afterwards the reluctance of chemists to adopt the new theory of causticity, gives as their excuse, that although this doctrine "admirably tallies with all the phenomena, yet it ascribes to fixed air properties which really make it a new body or existence" (*"forment réellement un nouvel etre"*).*

In order to estimate the importance of this discovery, and at the same time to show how entirely it altered the whole face of chemical science, and how completely the doctrine was original, we must now examine the state of knowledge which philosophers had previously attained upon the subject.

It has often been remarked that no great discovery was ever made at once, except perhaps that of logarithms; all have been preceded by steps which conducted the discoverer's predecessors nearly, though not quite, to the same point. Some may possibly think

* Supplement to the 'Encyclopédie,' vol. ii., p. 274, published in 1777.

that Black's discovery of fixed air affords no second exception to this rule; for it is said that Van Helmont, who flourished at the end of the sixteenth and beginning of the seventeeth century, had observed its evolution during fermentation, and given it the name of *gas silvestre*, spirit from wood, remarking that it caused the phenomena of the Grotto del Cane, near Naples. But though he as well as others had observed an aëriform substance to be evolved in fermentation and in effervescence, there is no reason for affirming that they considered it as differing from atmospheric air, except by having absorbed, or become mixed with, certain impurities. Accordingly, a century later than Van Helmont, Hales, who made more experiments on air than any other of the old chemists, adopts the commonly received opinion that all elastic fluids were only different combinations of the atmospheric air with various exhalations or impurities;* and this was the universal belief upon the subject, both of philosophers and of the vulgar.

It is now fit that we see in what manner the subject was treated by scientific men at the period immediately preceding Black's discoveries. The article 'Air' in the French 'Encyclopédie' was published in 1751, and

* It may safely be affirmed that Van Helmont's observation, which lay for a century and a half barren, threw no light of any value upon the subject. No one questions Newton's title to the discovery of the different refrangibility of light, and the true theory of the rainbow; yet, at the beginning of the 17th century, Antonio de Dominis, Archbishop of Spalatro, had really made an ingenious and well-grounded experiment on the similarity of the rainbow colours with those formed by the sun's rays refracted twice and reflected once in a globe filled with water. The doctrine of universal gravitation was known to both Kepler and Galileo; and Boulland (Astronomia Philolaica, lib. i., 1645), distinctly stated his belief or conjecture that it acted inversely as the squares of the distances. The famous proposition of equal areas in equal times was known to Kepler. The nearest approach to the Fluxional Calculus had been made by Harriott and Roberval and Fermat; and to take but one other example, the electrical explosion of the Leyden jar, discovered in 1747, obtained the name of the *coup-foudroyant*, and was by Abbé Nollet conjectured to be identical with lightning, Franklin's celebrated experiment being only made in 1752.

written by D'Alembert himself. It is, as might be expected, able, clear, elaborate. He assumes the substance of the atmosphere to be alone entitled to the name of air, and to be the foundation of all other permanently elastic bodies: "L'air élémentaire, ou l'air proprement dit," he says. He describes it as "homogène," and terms it "l'ingrédient fondamental de tout l'air de l'atmosphère, et qui lui donne son nom." Other substances or exhalations mix with it, he says, but these he terms "passagères," passing vapours, and not permanent: the air alone (that is, the atmospheric air) he calls "permanent," or permanently elastic (vol. i. p. 225). So little attention had the observation of Van Helmont respecting the Grotto del Cane excited, that we find a conjecture hazarded in the article 'Grotte' (vol. vii. p. 968), which appeared in 1756,—"peut-être respirent ils (les chiens), au lieu d'air, des vapeurs minérales;" but this was some time after Black's discovery had taught us to distinguish such permanently elastic vapours from atmospheric air. In the article 'Fermentation' (vol. vi. p. 523) we find Van Helmont's doctrines of the connection between fermentation and digestion treated with ridicule, and those who adopted them jocularly called the "fermentateurs."

A few years later, however, the face of things changed. In the 'Supplement,' published in 1776, we find an article on 'Fixed Air,' and a reference to Dr. Black's discovery; but nothing can be more indistinct than the author, M. Morveau's, ideas respecting it; for he leaves us in doubt whether it be the atmospheric air or a separate substance, and yet he states that the phenomena of fermentation and putrefaction are explained by the evolution or absorption of this air, and that mineral waters derive from its presence their flavour. An abstract of M. Venel's book had in 1765, under the head of 'Mineral Waters,' given this explanation; but instead of representing the air combined with the water as a different substance, he calls

it "véritable air et même très pure." We have, however, seen that, in the following year (1777), M. Morveau's ideas were perfectly distinct on the subject; for he treats it as a new substance, wholly different from atmospheric air. The slowness with which Black's doctrine made its way in France may be presumed from Morveau's remark on causticity, already cited, and also from this, that the article on 'Magnesia,' published in 1765, dogmatically asserts Black to be in error when he describes Epsom salts as yielding that earth, "because," says the author, "those salts are purely Seidlitian," "entièrement Seidlitiens" (vol. x. p. 858). In fact, Epsom salts, magnesia, limestone, and sea-water are the great sources from which all magnesia is obtained. The first of these substances is in truth only a combination of magnesia with sulphuric acid.

The other discoveries to which Black's led were as slowly disseminated as his own. Oxygen gas had been discovered, in August, 1774, by Priestley, and soon after by Scheele without any knowledge of Priestley's previous discovery; yet in 1777 Morveau, who wrote the chemical articles in the 'Supplement,' never mentions that discovery, nor the almost equally important discovery of Scheele, chlorine, made in 1774, nor that of azote, discovered by Rutherford in 1772, nor hydrogen gas, the properties of which had been fully investigated by Cavendish as early as 1766. Lavoisier's important doctrine, well entitled to be called a discovery, of the true nature of combustion, had likewise been published in 1774 in his 'Opuscules,' yet Morveau doggedly adheres to his own absurd theory of the air only being necessary to maintain those oscillations in which he holds combustion to consist; and finding that the increase of weight is always the result of calcination as well as combustion, he satisfies himself with making a gratuitous addition to the hypothesis of phlogiston, and supposes that this imaginary

substance is endowed with positive levity; nor does he allude to the experiments of Lavoisier on gases, on combustion, and on oxidation, further than to say that he had for a considerable time been engaged in these inquiries. It was not indeed till 1787 that he became a convert to the sound and rational doctrine, and abandoned the fanciful hypothesis, simple and ingenious though it be, of Stahl. Berthollet, the earliest convert, had come over to the truth two years before. Thus, discoveries had been made which laid the foundation of a new science, and on which the attention of all philosophers was bent; yet the greatest scientific work of the age made no more mention of them than if Black, Cavendish, Priestley, and Scheele had not been. The conjecture may be allowed to us, that if any of these great things had been done in France, M. Morveau would not have been suffered to preserve the same unbroken silence respecting them, even if his invincible prejudices in favour of the doctrine of phlogiston had disposed him to a course so unworthy of a philosopher.

The detail into which I have entered, sufficiently proves that the discovery of fixed air laid at once the foundation of the great events in the chemical world to which reference has just been made, because the step was of incalculable importance by which we are led to the fact that atmospheric air is only one of a class of permanently elastic fluids. When D'Alembert wrote the article 'Air,' in 1751, he gave the doctrine then universally received, that all the other kinds of air were only impure atmospheric air, and that this fluid alone was permanently elastic, all other vapours being only, like steam, temporarily aëriform. Once the truth was made known that there are other gases in nature, only careful observation was required to find them out. Inflammable air was the next which became the subject of examination, because, though it had long been known, before Black's discovery it had

been supposed only to be common air mixed with unctuous particles. His discovery at once showed that it was, like fixed air, a separate aëriform fluid, wholly distinct from the air of the atmosphere. The other gases were discovered somewhat later. But it is a very great mistake to suppose that none of these were known to Black, or that he supposed fixed air to be the only gas different from the atmospheric. The nature of hydrogen gas was perfectly known to him, and both its qualities of being inflammable and of being so much lighter than atmospheric air; for as early as 1766 he invented the air balloon, showing a party of his friends the ascent of a bladder filled with inflammable air. Mr. Cavendish only more precisely ascertained its specific gravity, and showed what Black could not have been ignorant of, that it is the same, from whatever substance it is obtained.

But great as was the discovery of fixed air, and important as were its consequences, the world was indebted to its illustrious author for another scarcely less remarkable, both from being so unexpected, and from producing such lasting effects upon physical science. About the year 1763 he meditated closely upon the fact, that on the melting of ice more heat seems to disappear than the thermometer indicates, and also that on the condensation of steam an unexpected proportion of heat becomes perceptible. An observation of Fahrenheit, on the cooling of water below the temperature of ice until it is disturbed, when it gives out heat and freezes at once, appears also to have attracted his careful consideration. He contrived a set of simple but decisive experiments to investigate the cause of these appearances, and was led to the discovery of *latent heat*, or the absorption of heat upon bodies passing from the solid to the fluid state, and from the fluid to the aëriform, the heat having no effect on surrounding bodies, and being therefore insensible to the hand or to the thermometer, and only by its absorption maintaining the

body in the state which it has assumed, and which it retains until, the absorbed heat being given out, and becoming again sensible, the state of the body is changed back again from fluid to solid, from aëriform to fluid. He never published any account of this discovery, but he explained it fully in his Lectures, both at Glasgow and Edinburgh, and he referred to it in the paper already mentioned, which was printed in the 'Philosophical Transactions' for 1775. Well, then, may we marvel that no mention whatever of latent heat is made in the celebrated 'Encyclopédie,' which owed its chemical contributions to no less a writer and experimentalist than Morveau. The doctrine of latent heat, however, was immediately applied by all philosophers to the production of the different airs which were successively discovered. They were found to owe their permanently elastic state to the heat absorbed in their production from solid or fluid substances, and to regain their fluid or solid state by combining either together or with those substances, and in the act of union giving out in a sensible form the heat which while absorbed and latent, had kept them in the state of elastic and invisible fluids.*

The third great discovery of Black was that which has since been called the doctrine of *specific heat*, but which he called the *capacity* of bodies for heat. Different bodies contain different quantities of heat in the same bulk or weight; and different quantities of heat are required to raise different bodies to the same sensible temperature. Thus, by Black's experiment, it was found that a pound of gold being heated to 150°, and added to a pound of water at 50°, the temperature of both became not 100°, the mean between the two, but 55°, the gold losing 95°, and the water gaining 5°,

* It is by no means impossible that one day we may be able to reduce the phenomena of light within the theory of latent heat. It may be that this body when absorbed, that is, fixed in substances, gives out heat; as, while passing through diaphanous bodies and remaining unfixed, its heat is not sensible.

because the capacity of water for heat is nineteen times that of gold. So twice as much heat is required to raise water to any given point of sensible heat as to raise mercury, the volumes of the two fluids compared being equal.

The true doctrine of combustion, calcination of metals, and respiration of animals, which Lavoisier deduced from the experiments of Priestley and Scheele upon oxygen gas, and of Cavendish on hydrogen gas, and which has changed the whole aspect of chemical science, was founded mainly upon the doctrines of latent and specific heat. It was thus the singular felicity of Black to have furnished both the pillars upon which modern chemistry reposes, and to have furnished them so long before any one attempted to erect the superstructure, that no doubt could by any possibility arise respecting the source of our increased knowledge, the quarter to which our gratitude should be directed. Fixed air was discovered in 1752, and fully explained to the world in 1754 and 1755. Latent heat was yearly, from 1763, explained to numerous classes of students, before whom the experiments that prove it were performed by the author's own hands. Cavendish made his experiments on inflammable air in 1766; Priestley began his in 1768, first publishing in 1772; and he discovered oxygen in 1774, in which year the nature of combustion was first explained by Lavoisier, a boy at school when fixed air was discovered, and having made no experiments nor written any one line upon chemical subjects for seven years after latent heat was discovered.

But we shall form a more striking idea to ourselves of the revolution which Black thus effected in chemistry, if we attend a little to the state of that science in general before he began his labours. We have already seen the low condition of the knowledge then possessed respecting aëriform fluids; the general condition of the science was in the same proportion humble.

The celebrated 'Preliminary Discourse,' to the 'Encyclopédie' makes hardly any mention of chemistry among the sciences; and in the 'Arbre Encyclopédique,' on which the authors (D'Alembert and Diderot) plume themselves much, we find it not very distinctly represented, or in very good company. It is termed the science of interior and occult qualities of bodies, its objects being to imitate and rival nature, by decomposing, reviving, and transferring substances. It is represented as holding among the sciences the place which poetry occupies among other branches of literature. Its fruits are said to be alchemy, metallurgy, natural magic, and chemistry properly so called, which is stated to consist of pyrotechny and dyeing. Strange to tell, pharmacy is not given as one of its fruits, being referred wholly to the branch of medical science.

But the state of chemistry is better understood by the article itself in the 'Encyclopédie,' the elaborate work of M. Venel of Montpelier, well known for his researches concerning mineral springs, and author of most of the chemical articles in the original work, as M. Morveau was of those in the 'Supplement,' and whose mistakes on the subject of magnesia, arising from prejudice, have already been mentioned. He begins this article with lamenting the low condition of his favourite science; "Elle est peu cultivée parmi nous. Cette science n'est que très médiocrement répandue, même parmi les savans, malgré la prétention, à l'universalité des connaissances qui font aujourd'hui le goût dominant. Les chemistes forment un peuple distinct, très-peu nombreux, ayant sa langue, ses mystères, ses loix, et vivent presque isolés au milieu d'un grand peuple peu curieux de sa connaissance, n'entendant presque rien de son industrie." He then goes on to show that this "incuriosité, soit réelle, soit simulée," is yet extremely unphilosophical, inasmuch as it leads to a rash condemnation; and that those who know any

subject superficially may possibly be deceived in their own judgment upon it, "the consequence of which has been," he adds, "that owing to the prejudices entertained against the nature and reach of the science, it becomes a matter of no small difficulty or slight controversy to say clearly and precisely what chemistry is. Some make no distinction between the chemist and the quack who seeks after the philosopher's stone *(souffleur)*; others think any one a chemist who has a still for preparing perfumes or colours. Many consider the compounding of drugs as containing the whole of the art. Even men of science know scarcely any thing about the chemists."—"What natural philosopher," he asks, "so much as ever names Becker or Stahl? Whereas those who, having other scientific illustrations, as John Bernouilli and Boerhaave, have written chemical works, or rather works on chemical subjects, are very differently thought of; so that the former's work on 'Fermentation,' and the latter's on 'Fire,' are known, cited, and praised, while the far greater views of Stahl on the same subjects only exist for a few chemists." He then goes on to cite other proofs of the low estimate formed of the science, and even the prevailing impression of chemists being mere workmen; and concludes, that "the revolution which should raise chemistry to the rank it merits, and place it on a level with natural philosophy, can only be accomplished by a great, an enthusiastic, and a bold genius." While waiting for the advent of this new Paracelsus, he says, it must be his task to present chemistry in a light which may show it worthy the notice of philosophers, and capable of becoming something in their hands.

If we go back to an earlier period, we shall find that Lord Bacon, although he quite clearly perceived that chemistry might one day be advanced to the rank of a science (De Dig. et Aug. iii.), yet always treats the chemistry of his day as merely empirical (Nov. Org.

s. lxiv. lxxiii.*). But I have preferred taking the account of chemical science from the 'Encyclopédie,' first, because it gives, if not the opinion or the testimony of the learned body at large who prepared that work, yet certainly an opinion and a testimony which had the sanction of its more eminent members; and, secondly, because its date is at the eve of the great revolution in natural science of which we are speaking. The last passage which has been cited from that work strikingly illustrates the low ebb at which chemical science then was.—It is certain that after the discoveries of Black had opened vast and new views of nature, both as regards the operations of heat, the most powerful and universal of all agents, and as regards the constitution of elastic fluids, the most unknown of the four elements, no natural philosopher would have had the hardihood to doubt if chemistry was an important branch of his science, and no chemist would have performed the superfluous task of vindicating its claim to the title.

* "Itaque talis philosophia (in paucorum experimentorum argutiis et obscuritate fundata) illis qui in hujusmodi experimentis quotidie versantur atque ex ipsis phantasmatis contaminarunt, probabilis videtur, et quasi certa; cæteris incredibilis et vana, cujus exemplar notabile est in chemicis eorumque dogmatibus."

It must be added that beside the injustice here done to Van Helmont, he goes on to rank Gilbert in the same empirical class, as he elsewhere does —a most incorrect view of Gilbert's induction, the most perfect by far of any before Lord Bacon's age, and, though mixed with some hypothetical reasoning, hardly in strictness excelled by any philosopher of after times. I cannot come so near the remarkable sixty-fifth section of the 'Novum Organum' without digressing so far from my subject as to cite the prophetic warning given to some zealots without knowledge of our own times against the "apotheosis errorum," the "pestis intellectus, si vanis accedat veneratio." "Huic autem vanitati (adds the pious and truly Christian sage) nonnulli ex modernis summâ levitate ita indulserunt ut in primo capitulo Geneseos et in libro Job et aliis scripturæ locis philosophiam naturalem fundari conati sunt; inter mortua quærentes viva;" a folly the more to be deprecated, he says, because "ex divinarum et humanarum malesana admistione non solum educitur philosophia phantastica, sed etiam religio hæretica." His practical conclusion, therefore, is to render unto faith the things alone which are faith's: "Admodum salutare, si mente sobriâ, fidei tantum dentur quæ fidei sunt."

We have now gone through the whole of this interesting subject, rather occupied in contemplating the foundations of a new science than in tracing the extension of the boundaries which confine an old one. The universal operation of heat, and the agency which, by its absorption and its evolution, it exerts on the structure of all bodies, renders the discovery of its nature and action in these respects, next to that of gravitation, the most important step which has been made in the progress of physical science. The new field opened to philosophical inquiry by the discovery of the gaseous bodies is only second to the former step in the importance of its consequences. It is as objects of pure science, the mere contemplation of scientific truth, that we have been considering these great discoveries; yet they have amply contributed also to the advancement of the arts. The illustrious improver of the steam engine was too young to have joined in the experiments on fixed air; but in the course of those by which latent heat was discovered, he had a constant and confidential intercourse with Black, one of his earliest patrons; and although it is as certain that he did not owe to that philosopher's suggestions any of the steps by which his inventions were compassed, as it is that he had himself no share in Black's great discovery, it cannot be doubted that the knowledge thus acquired of the true nature of heat, of steam, of evaporation, and of condensation, contributed most essentially to his mighty improvements. As for the gases, it would be difficult to name the branch of art which has not in some manner and to some extent gained by their discovery. So that the great man whose history we are contemplating, had the satisfaction of seeing the triumphs of his youth bear fruit in every direction, exalting the power and increasing the comforts of mankind as well as extending the bounds of their knowledge and enlarging the range of their industry. He was but twenty-four years old when he made his first discovery,

and thirty-four when his second was added. He lived to nearly fourscore.

It remains to consider him as a teacher; and certainly nothing could be more admirable than the manner in which for forty years he performed this useful and dignified office. His style of lecturing was as nearly perfect as can well be conceived; for it had all the simplicity which is so entirely suited to scientific discourse, while it partook largely of the elegance which characterized all he said or did. The publication of his lectures has conveyed an accurate idea of the purely analytical order in which he deemed it best to handle the subject with a view to instruction, considering this as most likely to draw and to fix the learner's attention, to impress his memory, and to show him both the connection of the theory with the facts, and the steps by which the principles were originally ascertained. The scheme of the lectures may thence be apprehended—the execution imperfectly; for the diction was evidently, in many instances, extemporaneous, the notes before the teacher furnishing him with little more than the substance, especially of those portions which were connected with experiments. But still less can the reader rise from the perusal to any conception of the manner. Nothing could be more suited to the occasion; it was perfect philosophical calmness; there was no effort; it was an easy and a graceful conversation. The voice was low, but perfectly distinct and audible through the whole of a large hall crowded in every part with mutely attentive listeners; it was never at all forced any more than were the motions of the hands, but it was anything rather than monotonous. Perfect elegance as well as repose was the phrase by which every hearer and spectator naturally, and as if by common consent, described the whole delivery. The accidental circumstance of the great teacher's aspect, I hope I may be pardoned for stopping to note, while endeavouring to convey the idea of a philosophic

discoverer. His features were singularly graceful, full of intelligence, but calm as suited his manner and his speech. His high forehead and sharp temples were slightly covered, when I knew him, with hair of a snow-white hue, and his mouth gave a kindly as well as most intelligent expression to his whole features. In one department of his lecture he exceeded any I have ever known, the neatness and unvarying success with which all the manipulations of his experiments were performed. His correct eye and steady hand contributed to the one; his admirable precautions, foreseeing and providing for every emergency, secured the other. I have seen him pour boiling water or boiling acid from a vessel that had no spout into a tube, holding it at such a distance as made the stream's diameter small, and so vertical that not a drop was spilt. While he poured he would mention this adaptation of the height to the diameter as a necessary condition of success. I have seen him mix two substances in a receiver into which a gas, as chlorine, had been introduced, the effect of the combustion being perhaps to produce a compound inflammable in its nascent state, and the mixture being effected by drawing some string or wire working through the receiver's sides in an air-tight socket. The long table on which the different processes had been carried on was as clean at the end of the lecture as it had been before the apparatus was planted upon it. Not a drop of liquid, not a grain of dust remained.

The reader who has known the pleasures of science will forgive me if at the distance of half a century I love to linger over these recollections, and to dwell on the delight which I well remember thrilled me as we heard this illustrious sage detail, after the manner I have feebly attempted to pourtray, the steps by which he made his discoveries, illustrating them with anecdotes sometimes recalled to his mind by the passages of the moment, and giving their demonstration by

performing before us the many experiments which had revealed to him first the most important secrets of nature. Next to the delight of having actually stood by him when his victory was gained, we found the exquisite gratification of hearing him simply, most gracefully, in the most calm spirit of philosophy, with the most perfect modesty, recount his difficulties, and how they were overcome; open to us the steps by which he had successfully advanced from one part to another of his brilliant course; go over the same ground, as it were, in our presence which he had for the first time trod so many long years before; hold up perhaps the very instruments he had then used, and act over again the same part before our eyes which had laid the deep and broad foundations of his imperishable renown. Not a little of this extreme interest certainly belonged to the accident that he had so long survived the period of his success—that we knew there sat in our presence the man now in his old age reposing under the laurels won in his early youth. But take it altogether, the effect was such as cannot well be conceived. I have heard the greatest understandings of the age giving forth their efforts in its most eloquent tongues—have heard the commanding periods of Pitt's majestic oratory—the vehemence of Fox's burning declamation—have followed the close-compacted chain of Grant's pure reasoning—been carried away by the mingled fancy, epigram, and argumentation of Plunket; but I should without hesitation prefer, for mere intellectual gratification (though aware how much of it is derived from association), to be once more allowed the privilege which I in those days enjoyed of being present while the first philosopher of his age was the historian of his own discoveries, and be an eye-witness of those experiments by which he had formerly made them, once more performed with his own hands.

The qualities which distinguished him as an inquirer

and as a teacher followed him into all the ordinary affairs of life. He was a person whose opinions on every subject were marked by calmness and sagacity, wholly free from both passion and prejudice, while affectation was only known to him from the comedies he might have read. His temper in all the circumstances of life was unruffled. This was perceived in his lectures when he had occasion to mention any narrow prejudice or any unworthy proceeding of other philosophers. One exception there certainly was, possibly the only one in his life; he seemed to have felt hurt at the objections urged by a German chemist called Meyer to his doctrine of causticity, which that person explained by supposing an acid, called by him *acidum pingue*, to be the cause of alkaline mildness. The unsparing severity of the lecture in which Black exposed the ignorance and dogmatism of this foolish reasoner cannot well be forgotten by his hearers, who both wondered that so ill-matched an antagonist should have succeeded where so many crosses had failed in discomposing the sage, and observed how well fitted he was, should occasion be offered, for a kind of exertion exceedingly different from all the efforts that at other times he was wont to make.

The soundness of his judgment on all matters, whether of literature or of a more ordinary description, was described by Adam Smith, who said, he "had less nonsense in his head than any man living." The elegance of his taste, which has been observed upon as shown in his lectures, was also seen in the efforts of his pencil, which Professor Robison compares to that of Woollett. The neatness of his manipulations was not confined to his experiments when investigating or when lecturing. I have heard one who happened to see him at his toilette describe the operations as performed with exquisite neatness by a number of contrivances happily adapted to the saving of trouble and avoiding uneasiness. His perfect equanimity has been

adverted to, and it did not proceed from coldness of disposition, for he was affectionately attached to his friends. Having no family of his own, he may be said to have fallen into those precise and regular habits which sometimes raise in happier individuals a smile, I stop not to inquire whether of envy or contempt, for the single state. It was sometimes said, too, that his habits were penurious. That the expenses of one who had no love of pleasure and no fancy for ostentation to gratify, must have been moderate, is certain; but he lived in the style and manner suited to one possessing an ample income. The ground of the charge was, I believe, that he was said to have a scale by him when he received the fees of his students. I can answer for the truth of this statement, for I well remember the small brass instrument; but I also recollect that he said it became necessary from the quantity of light gold which he used at first to receive unsuspected from one class, particularly, of his pupils. There was certainly no reason why he should pay a sum of forty or fifty pounds yearly out of his income on this account. Both Professor Ferguson and Professor Robison have positively denied the charge of avarice, and have given ample testimony even to his generous nature. While he lived at Glasgow he lost three-fourths of his fortune by the failure of a house in which it was invested; and though he had foreseen the catastrophe for two years, he neither attempted to withdraw his funds, nor altered in any respect his kind demeanour towards the head of the firm, whom he knew. At Edinburgh he more than once incurred great risks to help friends in business.

The gradual decay of his strength brought about the extinction of life without pain and without any discomposure. Professor Robison told me that he was sure nothing could be more agreeable to his illustrious friend's wishes than this end, as nothing was more likely to vex and annoy him than the unavoidable

accompaniments of a protracted illness and a sick-bed. He often indeed expressed a wish that he might be spared this suffering, and that wish was fully gratified. It seemed, said the Professor, as if he waited calmly until the last stroke of his pulse should be given. It is certain that he passed from this life so quietly as not to spill a cup of milk and water (a customary dinner with him) which he at the moment was holding in his hand, and which rested on his knee. His attendants saw him in this posture, and left the room supposing him still alive. On returning soon after they saw him exactly sitting as before, and found that he had expired.

WATT.

The intimacy of Mr. Watt with Dr. Black from his earliest years has been already mentioned. When the latter was a Professor at the University of Glasgow, Watt, then a young man, was employed as mathematical instrument maker to the Natural Philosophy class, and was in daily communication with the Professor while his experiments on heat, evaporation, and condensation were carried on. I well remember him afterwards, in his lectures at Edinburgh, mentioning that his young coadjutor employed himself at the same time in researches upon the nature of steam; and it is certain that his subsequent inventions were greatly aided by the discoveries of Black respecting heat. To the inquiries out of which these inventions arose, he appears to have been led by the accident of having a model of an engine to repair for the Professor of Natural Philosophy. But, before examining the foundations upon which his great and well-earned fame rests, it is fit that we should first consider the state in which he found the engine, which he almost created anew. This is following the same course which has been pursued with respect to the discoveries of Dr. Black.

The power of steam is far too generally perceived in the ordinary affairs of life to have wholly escaped the observations of men at any period. The ancients accordingly were so far acquainted with it as to have constructed an instrument, the æolipile, composed of a metallic ball, which having some water in its bottom, was placed in the fire, and the steam issuing through a

small orifice or tube with great force, could, they conceived, blow a fire or even turn the vanes of a mill. No use, however, seems ever to have been made of this philosophical toy, nor does any attention appear to have been paid to steam, as an agent, until 1615, when Salomon de Caus, a French engineer, published a work on 'Moving Forces,' in which he describes a method of raising water by partially heating it, that is, converting a portion of it into steam, and, by its expansive force, driving the rest of the fluid through the tube connected with the reservoir or chamber.* In 1663 the Marquis of Worcester (known in our political history as Earl of Glamorgan, and as having been employed by Charles I. in 1646 to negotiate with the Irish Catholics) published his 'Century of the Names and Scantlings of Inventions,' of which Mr. Hume, in his 'History' (vol. vii., note *o*), has been pleased to say that it is "a ridiculous compound of lies, chimeras, and impossibilities, showing what might be expected from such a man." The better opinion seems to be, that the historian had never read the book he thus describes; but being anxious to relieve Charles I., from the blame of his Ambassador's negotiation, which proved the source of much outcry against the King, he states the low opinion which the latter entertained of Worcester's judgment as a proof that he never would authorize him to act in so delicate a matter as religious concessions without the privity of the Lord-Lieutenant, and he is very ready to strengthen this view by showing that the opinion was well founded. Be this, however, as it may, the ignorance and error is all on Hume's

* M. Arago is not entitled to complain of English writers for having "aimed at expunging every French name from this important chapter in the history of science." He says they at once gave up Lord Worcester's claims on discovering that Salomon de Caus had preceded him. Now, both Mr. Farey and Mr. Stuart have done ample justice to Caus in their works on the steam engine. As for Lord Worcester, Mr. Stuart (whose history is far from accurate on this point) has both attacked and defended his claims in his several works.

side, for the work is highly creditable to its author's learning and ingenuity, and it undoubtedly contains a proof that he had made one step in advance of Caus towards the use of steam power. His Sixty-eighth Invention is entitled "an admirable and most forcible way to drive up water by fire." He describes his having made a "constant fountain stream of water, raised in the proportion of forty times the quantity of that which he converted into steam;" and he states that the height to which he raised it was forty feet, clearly showing that it was not on the principle of the sucking-pump, which can only raise water thirty-three feet. He expressly says, that while the atmospheric pressure by which the sucking-pump acts is limited in its operation, the force of steam which he employed "hath no other boundary than the strength of the vessel which contains it." Finally, he seems to have used a cannon as his boiler, which indicates his having tried the experiment on a large scale. The great doubt expressed by M. Arago, whether or not Lord Worcester ever executed the design more or less clearly described in his book, appears to me to have no foundation. The inference arising from the description seems to remove that doubt; but we have external evidence more precise and satisfactory still.* The travels in England of the Grand Duke of Tuscany, Cosmo de' Medicis, were written by his Secretary, Magalotti, a man of some scientific eminence; and a translation into English was published in 1821. The visit to London took place in the year 1669; and it appears that the Grand Duke "went to see, at Vauxhall, an engine or hydraulic machine invented by the Marquis of Worcester," and the account which he gives of it tallies with Lord Worcester's description of his "stupendous water-commanding engine."

* See also the Marchioness of Worcester's correspondence with her Confessor, communicated by the Beaufort family to Mr. Partington for his edition of the "Century."

The account of Lord Worcester is far from being clear and distinct, and nothing appears to have resulted from his suggestions. In 1690, Papin, an eminent and able French engineer and author of the digester which goes under his name, living in London, published a work in which he showed that he had made two most important steps in the use of steam. Caus and Worcester had applied the force directly to the body which it was intended to move; and it was evident that, while that was a condition of its use, very limited bounds must confine the operation. But Papin, observing the use of the piston in a common sucking-pump, applied this to the steam machine, making it work in the cylinder, and be the medium of communicating motion to other apparatus. Next, he applied steam directly as the agent, to raise the piston; and making a vacuum *by the condensation of the steam*, he thus caused the atmosphere to press down the piston. Guericke, the inventor of the air-pump, had half a century earlier used the vacuum, made by his machine in the same manner, as a mechanical power, by the help of a piston and rod;* and he invented the valve, without which the vacuum could not be produced. The application of the same principle and of the same contrivance to steam was Papin's; and its importance, and his merit, are not diminished by considering the source from which he borrowed it.† Indeed the action of the air in the sucking-pump is another form of the same experiment. It must be added that to Papin also we owe the important invention of the safety-valve, although he did not apply it to the steam engine. He introduced it as a part of his digester, but suggesting that it was applicable to the steam engine.

* See the distinct figure in his plate xiv., p. 109, of Experimenta nova Magdeburgica de Vacuo Spatio.' Amstelodami, 1672.

† Acta Eruditorum, 1688. The paper has an excellent and clear figure. Nothing can be more groundless than Mr. Stuart's statement that Baptista Porta had anticipated Papin in this important step. The passage refers only to the rise of water in a vacuum. See 'I tre Libri dei Spiritali,' 1606.

It is, however, certain that the most rude and cumbrous part of the former invention was continued by Papin. The fire was applied to the water, and when it had filled the cylinder with steam, the condensation was only effected by withdrawing or extinguishing the fire. Savery about the year 1698 made considerable improvements on the apparatus; and though he did not use the vacuum as Papin had done, but only as it is used in the sucking-pump, he yet produced it by applying cold water to the outside of the cylinder. The machines made by him were so manageable that they were brought into use for raising water in many country houses. D'Alesme exhibited a machine before 1705 (as appears by the 'Histoire de l'Académie des Sciences' for that year, p. 137), in which water was made to spout to a great height by the force of steam alone.

It is extremely doubtful if Papin ever erected any steam engine, either upon his own or upon any other principle. It is certain that he did not adhere to the two great propositions which he had brought forward, the operating by a piston, and the operating by the pressure of the atmosphere; he recurred to the old plan of making the steam act directly upon the weight to be raised. In 1711 Newcomen, an iron-master of Dartmouth, and Calley, or Cawley, a glazier of the same town, constructed an engine upon Papin's principle of a piston and a condensing process, using, however, Savery's mode of creating a vacuum by cold affusion, for which they were led by an accident to substitute the method of throwing a jet or stream of cold water into the cylinder. This important improvement saved, in a considerable degree, the waste of heat occasioned by Savery's method of condensing. Their engine could be applied with advantage to raise water from mines, which Savery's was wholly incapable of effecting, its power being limited to that of the sucking-pump. Newcomen's engine, as it is generally called,

made no use at all of the direct force of steam; it worked entirely by means of the vacuum; and hence it is sometimes and justly termed the atmospheric engine, as its moving force is the pressure of the atmosphere. Desaguliers, who has given the best description of Newcomen and Cawley's engine, about the year 1717 or 1718 made several of those engines, in which he executed Papin's suggestion of using the safety-valve. In the same year Beighton perfected the mechanism whereby the engine itself shut and opened the valves, by which the supply of steam to the cylinder and of water to the boiler is regulated; and Smeaton subsequently made some other mechanical improvements. With these exceptions the steam engine continued exactly in the same state from the time of Newcomen to that of Watt, above half a century later.

We have thus seen how very slowly this great invention was brought to the state in which Mr. Watt found it, and how considerable a number of persons contributed each a small share to its progress. Let us enumerate these steps: they are at least six in number. S. de Caus made steam act to raise water; Worcester performed this operation in a more regular and mechanical manner; Papin used the condensation of steam, and through that the atmospheric pressure, as well as the direct expansive force, and he worked the engine by a piston; Savery condensed by refrigeration instead of the mere absence of fire, but did not use the atmosphere; Newcomen used the jet for condensing and the atmosphere for pressure, but did not use the direct force of steam; Desaguliers introduced the safety-valve; Beighton and Smeaton improved the mechanism; D'Alesme needs not be mentioned, as we are not informed what plan he executed, but he certainly made no step himself. If the direct force of steam, as well as atmospheric pressure, had been both employed, with the jet of cold water, the safety-valve, and the contrivance for regulating the supply-valves, a far better engine than any

ever known before the time of Watt would have been produced, and yet nothing whatever would have been added to the former inventions; they would only have been combined together. The result of the whole is, that one of the greatest theoretical steps was made by Papin, who was, during a long period, little commemorated; and that Savery and Newcomen, who have been by many called the inventors, were the first of all the ingenious and useful persons whose successive improvements we have now recorded, to apply the steam-engine to practical purposes. France has thus produced the man who, next to Watt, must be regarded as the author of the steam-engine; of all Watt's predecessors, Papin stands incontestably at the head; but it is almost certain that he never actually constructed an engine. Though the engine of Savery was of considerable use in pumping to a small height, and indeed has not entirely gone out of use even in our own times, and though Newcomen's was still more extensively useful from being applicable to mines, not only had no means ever been found of using the steam power for any other purpose than drawing up water, but even in that operation it was exceedingly imperfect and very expensive, insomuch that a water power was often preferred to it, and even a horse power in many cases afforded equal advantages. The great consumption of fuel which it required was its cardinal defect; the other imperfection was its loss of all direct benefit from the expansive force of the steam itself. That element was only used in creating a vacuum, and an air-pump might have done as much had it been worked by water or by horses. It was, in the strictest sense of the word, an air and not a steam engine.

When Mr. Watt was directed to repair the working model for the Professor at Glasgow, he of course examined it attentively. He was at that time, 1763, in his twenty-eighth year, having been born in 1736 at Greenock, where his father was a magistrate, and he

had learnt the business of a mathematical instrument maker. He had been prevented by delicate health from benefiting much by school instruction; but he had by himself studied both geometry and mechanics, having from his childhood showed a marked taste for those pursuits, in which his grandfather and uncle, teachers of the mathematics, had been engaged. It is related of him that a friend of his father's one day found the child stretched on the floor drawing with chalk numerous lines that intersected each other. He advised the sending the young idler, as he supposed him, to school, but the father said, "Perhaps you are mistaken; examine first what he is about." They found he was trying, at six years old, to solve a problem in geometry. So his natural turn for mechanics was not long in showing itself; and his father indulging it by putting tools in his hands, he soon constructed a small electrical machine, beside making many childish toys.

He occasionally visited his mother's relations at Glasgow, but never attended any lectures there, or elsewhere. The ardour of his active mind was superior to all the restraints which the weakness of his bodily frame could impose. He devoured every kind of learning. Not content with chemistry and natural philosophy, he studied anatomy, and was one day found carrying home for dissection the head of a child that had died of some hidden disorder. His conversation, too, was so rich, so animated, that we find, from the relation of Mrs. Campbell, a female cousin of his, the complaints made by a lady with whom he resided. She spoke of the sleepless nights which he made her pass by engaging her in some discussion, or some detail of facts, or some description of phenomena, till the night was far advanced towards morning, and she found it impossible to tear herself away from his talk, or to sleep after he had thus excited her.

In 1755 he placed himself with Mr. Morgan, mathematical and nautical instrument maker, of Cornhill, and

resided with him somewhat less than a year, during which he was chiefly employed in the preparation and adjustment of sextants, compasses, and other nautical instruments. But the same feeble health which had interrupted his studies at Glasgow again oppressed him; he was obliged to leave London, and return to Glasgow. On his arrival there he had the intention of setting up as an instrument maker, but the Glasgow Body of Arts and Trades, one of the sub-corporations in the municipal corporation of that city, opposed him as not free of their craft or guild, and therefore not entitled to exercise his calling within the limits of the charter. Attempts were made to obtain their leave for a very small workshop, on the humblest scale, but this was peremptorily refused. The University therefore came to his assistance, granted him a room in their own building, and gave him the appointment of their mathematical instrument maker. There remain small instruments then made by him in this workshop, and executed entirely with his own hands; they are of exquisite workmanship. The earliest of his steam-engine drawings are likewise preserved, and those competent judges who have examined them, particularly M. Arago, describe them as "truly remarkable for the neatness, the strength, and the accuracy of their outline." His manual dexterity and skill, therefore, is clear, and he had good cause to plume himself as he always did upon it, estimating the same quality in others at its just value.

In the course of a very few years, beside renewing his intimacy with Mr. Robison, afterwards Professor there and at Edinburgh, he became intimately acquainted with the most eminent of the Glasgow Professors, Adam Smith, Robert Simson, Robert Dick, and above all, Dr. Black. Of these all but Mr. Dick have left the deep impress of their great names upon the scientific history of their age; and he was always described by both Mr. Watt and Professor Robison as a person of most admirable capacity and great attain-

ments, treating natural philosophy, too, with singular ability and success, nor prevented from acquiring a more extensive and lasting reputation by anything save his premature death.

While thus occupied and thus befriended by men of great names, his own reputation increased daily as a successful cultivator of natural science. His workshop became the resort of all zealous students and enlightened inquirers into physical science, and was particularly resorted to by the pupils of the University. Professor Robison tells us that though regarding himself as a proficient in the mixed mathematics and in experimental philosophy, he was somewhat mortified at finding Watt so greatly his superior in the same favourite departments of study. In truth, it was the ordinary practice to consult him as the oracle upon any difficulty coming in the way of either students or inquirers. His fixed resolution to be deterred by no difficulties was constantly apparent, and one example is given by the Professor. The solution of a problem which occupied Watt and his friends, seemed to require the perusal of Leupold's Theatre of Machines, and as it was written in German, he at once learnt that language in order to consult the book. Another instance of his indomitable perseverance against great difficulties apparently irremovable, though not insuperable, may be added. He had no ear at all for music: not only was he through life wholly insensible to its charms, but he could never distinguish one note from another; yet he undertook the construction of an organ: and the instrument which he made not only had every mechanical merit from the most ingenious contrivances for conducting and regulating the blasts and the movements of the machine, but produced the most admirable harmonic results, so as to delight the best performers. He overcame the difficulties which lay in his way, partly by the phenomenon of the beats of imperfect consonances, a theory then little understood, and only

contained in a work at once very profound and very obscure, Smith's 'Harmonics.' This treatise, of which only the first and less perfect edition was then published, must have been read and understood by the young engineer. While employed by Dr. Roebuck at his Works, he made a guitar for his daughter, afterwards Mrs. Stuart, which she still possesses, and relates the sum given for it to have been five guineas.

It only remains to add that all the reading and all the speculations of Watt were strictly confined to hours which did not interfere with his profession or his trade of an instrument maker. The whole of the day was devoted to his business, only subject to the interruption of the discussions raised by those who frequented his workshop in search of assistance and information. It was late in the evening, or rather in the night, that he prosecuted with zeal and close attention his philosophical studies; for his principle through life was steadily kept in view, and uniformly acted on, never to let anything whatever interfere with business, the transaction of which he regarded as a primary duty to be performed, and entitled, as such, to take precedence of all other pursuits.

There chanced to be among the apparatus of the Natural Philosophy class a model of Newcomen's steam-engine, which, from some defect in the construction never could be got to work well; and Mr. Watt was desired to examine and report to the Professor, Mr. Anderson, successor of Dr. Dick, and better known afterwards as having founded by his will the class in which Dr. Birkbeck taught the working men, and thus gave rise to Mechanics' Institutes. The construction of this working model was found to be exceedingly imperfect, but Mr. Watt soon remedied all its defects. As far as the kind of engine could answer its purpose, the apparatus was found to perform its functions satisfactorily, being annually exhibited to the class with great success. He had, however, been taught by his

examination of the model what were the defects of the machine itself, and which no care in repairing or adjusting that model could remove. He found first of all that the boiler was much too small in proportion to the column of water which the steam had to raise, and yet it was larger than the boiler used in practice. The cylinder was on the scale of two inches diameter, the height being half a foot. The vacuum being imperfect from the size of the boiler, he diminished the length of the piston-rod. He found that the brass of which the cylinder was made carried off a great deal of heat, and that too large a surface was exposed to the steam. These observations set him upon making a variety of experiments upon steam, and upon the mode of applying it both directly and to produce a vacuum. He had, in the year 1759, while a fellow-student with Mr. Robison, received from that gentleman a suggestion of the application of steam to wheel-carriages, as he tells us in 1803, long before steam travelling was dreamt of.* They had together made experiments on Papin's digester, in order to ascertain the expansive force of steam; but these speculations had for several years been given up. In 1760 and the two following years Watt had been in familiar intercourse with Professor Black, had witnessed his experiments on heat, and had learnt from him the true cause of evaporation and condensation. When, therefore, he began to experiment upon the mechanical application of steam, its expansion, and its condensation, he enjoyed that inestimable advantage of thoroughly knowing the principles on which its changes and its action depended. His own experiments now put him in possession of the causes which determine the rapidity of evaporation, the proportion which it bears to the surface exposed

* Mr. Murdock, in 1784, made a working model of a steam-carriage, which moved about the room. It was constructed upon the principle set forth in Mr. Watt's specification of 1769, Art. iv., and this is the very method used at the present day

to the fire, the effects of pressure upon the boiling-point, and the quantity of fuel required to convert a given quantity of water into steam—circumstances which had hitherto been only vaguely and generally examined, but which he now reduced to mathematical precision.

The first discovery which he made upon the atmospheric engine and its waste of fuel, was that the injection of cold water which condenses the steam also cools the cylinder to a degree which requires a great expenditure of fuel again to give it the necessary heat for keeping the steam expanded to fill it. He found that three-fourths of the fuel employed were thus consumed; in other words, that if the cylinder could be kept at the temperature which it has before the jet is thrown in, one-fourth of the fuel would suffice for the operation.

The next defect of the process was scarcely less important. The water injected, coming in contact with the steam, was itself heated; the evolution of the latent heat, which Black's discovery showed Watt necessarily took place on its condensation, had the effect, together with the absorption of the steam's sensible heat, of converting a portion of the injected water itself into steam. Hence the vacuum was very far from perfect; and the resistance which the piston thus met with in its descent was found to be equal to one-fourth part of the atmospheric pressure, that is to say, the working power of the machine was diminished one-fourth.

From the distinct view thus obtained of the evil arose the suggestion of the remedy. The whole mischief proceeded from the condensation being performed in the cylinder, where the steam was thrown and the piston worked. It occurred to Watt, that if the condensation could be performed in a separate vessel, communicating with the cylinder, the latter could be kept hot while the former was cooled, and the vapour arising from the injected water could also be prevented

from impairing the vacuum. The communication could easily be effected by a tube, and the water could be pumped out. This is the *first* and the grand invention by which he at once saved three-fourths of the fuel, and increased the power one-fourth, thus making every pound of coal consumed produce five times the force formerly obtained from it. But this was not all. He found it expedient to remove the air from the upper part of the cylinder, as it tended to diminish the heat. In effecting this he was, *secondly*, led to open a communication with the boiler, and introduce steam above the piston while it descended, thus making the upper chamber of the cylinder air-tight. The steam thus acted in aiding the descent of the piston, instead of that descent being accomplished merely by the atmospheric pressure. *Thirdly*—the counterpoise at the pump-rod was done away, as a mere loss of power, and the piston was now forced upwards by the steam entering to fill the cylinder. These two great additional improvements only required a communication to be opened by tubes with the condenser as well as the boiler, and they gave to the machinery its right to be called a steam-engine; for it now worked more by steam than by air. The upper chamber was kept air-tight by making the piston-rod work in a socket of tow saturated with grease, called the stuffing-box, which also diminished greatly the friction of the rod.

If Mr. Watt's invention had gone no further than this, we may perceive that it not only increased the power of the fuel fivefold directly, but obtained from the steam as much additional force as could be derived, the limit being only the strength of the materials, within which limit the safety-valve of Papin always enabled the engineer to keep his power. But the three particulars which have been described were not the whole of this great engineer's improvements upon the mechanism of his predecessors. The smooth working of the engine, especially if it be applied to other

and finer operations than those of the miner, depends essentially on the accurate position of the piston-rod, with whatever velocity moving, and against whatever weight contending. Its motion must be steadily maintained in the same vertical straight line, or in the same horizontal line, or in the same straight line whatever be its direction, without shaking or inclining so as to press at all against the sides of the cylinder—any such lateral pressure occasioning a loss of time, a jolting motion, a general derangement of the machinery. The motion of the rod and the piston must be perfectly equable, continuous, and smooth: it must work, as the engineers sometimes say, *sweetly*, at every instant, in order that the engine may well perform its functions. The contrivance for producing this motion of the rod so that it shall be always in one line parallel to some supposed line whether vertical, as in a mine, or horizontal, or in any other direction, is thence called the "*Parallel Motion*," and it is one of Mr. Watt's most exquisite discoveries, and one to which scientific principle has the most conduced. If a circle or other curve has its curvature gradually changed, until from being concave to its axis it becomes convex, it will pass through every possible position or variation (whence the great refinement upon fluxions, the *calculus of variations*, probably derived its name, if not its origin), and at one point it will be a straight line, or will coincide with a straight line. So if a curve have two branches, one concave to the axis, the other convex, as a cubic parabola for example, the point at which its concavity ends and its convexity begins, is called for that reason a point of contrary flexure. The contrivance of the parallel motion consists in making the contrary circular motions of arms which bear on the rod always keep to the point of contrary flexure and thus give a rectilinear motion to the rod, the tendencies to disturb it correcting each other. It was long ago shown by Sir

Isaac Newton, in the 'Principia,' that if a circle moves upon another of twice its diameter, each point describes a straight line. This is precisely the principle of the parallel motion. Three beams are made to revolve round different centres, two of these being moveable in the arm of the engine, and one fixed without it. These three are connected together and with the piston-rods of the cylinder and the pumps, which their revolutions cause to describe accurately straight lines.

A *fifth* invention is the *Float*, which, placed on the surface of the water in the boiler, descends until the water is so low as to require a supply; it then opens a valve which lets in the quantity wanted; for, as soon as it rises to a certain height, the valve is shut by the float.

The most refined contrivance of the whole may now be mentioned, in the *sixth* and last place, the adaptation of the *Governor*, previously used in wind and water mills. It is evident that the velocity of the working may be increased beyond what is required or convenient without the safety-valve giving any indication of the excess, and also that the warning given by this valve does little more than point out the risk without providing the remedy or preventive. The governor is a far more subtle invention. Two balls are fixed to the end of arms which are connected with the engine by a moveable socket; this can play up and down a vertical rod revolving by a band on the axis or spindle of the fly-wheel, and it revolves, therefore, with the velocity of that spindle. The arms are perfectly moveable on their centres, which are fixed in the socket and on opposite sides of it. Their centrifugal force, therefore, makes them diverge, more or less, in proportion as the rotatory motion of the spindle and consequently the velocity of the engine increases; their divergence pushes the collar up the spindle, its axis, and as it rises, it closes, by means of cranks, a valve called the "throttle-valve," in the pipe which conveys the steam from the boiler to the cylinder, and

this lessens the supply of steam; the motion of the engine is thus reduced, the centrifugal force is abated, the balls approach the spindle again, the collar descends, the throttle-valve is gradually opened, and the supply of steam again slowly increased, but never beyond the quantity required, because as soon as that is exceeded, the increase of centrifugal force causes the balls to diverge, the collars to rise, and the valve to close. Thus the engine itself provides for its continuing in the state of perfect adjustment required. As long as its motion continues uniform, the balls revolve at the mean distance from their axis without either receding or approaching, and the supply of steam continues the same. As soon as the motion becomes excessive, they diverge, and the supply of steam is diminished; as soon as the motion becomes defective, they converge, and the supply of steam is increased. But further, the balls themselves, by their increased motion, absorb part of the force, independent of their action on the throttle-valve, and so contribute to the adjustment.

The sagacious inventor soon satisfied himself that he had almost created a new engine of incalculable power, universal application, and inestimable value. But he had not the funds either to try his invention upon an adequate scale so as to bring it into use, or to secure his property in it by obtaining a patent. After some repulses, he happily met with Dr. Roebuck, a man of profound scientific knowledge, and of daring spirit as a speculator. He had just founded the Carron Iron Works, not far from Glasgow, and was lessee, under the Hamilton family, of the Kinneil Coal Works. He was the grandfather of the present Member for Bath, who, descended from him on the one side, and from the Tickells* on the other, may be said to unite in himself rare claims to hereditary distinction; but who is probably destined to exalt the name of his

* His maternal grandfather was the author of 'Anticipation,' and grandson of Addison's friend, the poet.

family still higher by his own virtues. Dr. Roebuck, like too many ingenious men, for the benefit of others founded these Carron and Kinneil Works; and though he agreed to Mr. Watt's terms of receiving two-thirds of the profit, he was obliged by pecuniary embarrassments to retire from the partnership after a patent had been obtained in 1769, and an engine of an eighteen-inch cylinder had been erected at Kinneil. The success of this amply proved the solidity of the invention, but the inventor was obliged, for some years, to abandon the pursuit, and to labour in his profession of what is now termed a civil engineer; but the extensive operations of which Scotland soon became the scene, gave a much more ample scope to his talents. He was actively engaged in the surveys, and afterwards in the works, for connecting by a canal the Monkland coal-mines with Glasgow. He was afterwards employed in preparing the canal since completed by Mr. Rennie, across the Isthmus of Crinan; in the difficult and laborious investigations for the improvement of the harbours of Ayr, Greenock, and Glasgow; in improving the navigation of the Forth and the Clyde; in the Campbelton Canal, and in the surveys and plans preliminary to the Grand Caledonian Canal; beside several bridges of great importance, as those of Hamilton and Rutherglen. At Dr. Roebuck's Mr. Watt had early received much kindness, and many valuable lessons in chemical science. He was here, too, introduced to Dr. Black.

The various works which have been mentioned occupied his whole time from the disappointment experienced in 1769 respecting the steam engine, of which during that long interval he never despaired, to the year 1774, when he acceded to the proposal of Mr. Boulton, of Soho, near Birmingham, that he should be taken in Dr. Roebuck's place as partner in the patent, and in 1775 he settled there in this new business. An extension of the patent for twenty-five

years from this time was obtained from Parliament, in consequence of the national importance which all men saw belonged to the invention; and the partners constructed many engines upon the terms of receiving one-third of the fuel saved by the improvements. It is a convenient mode of illustrating the effect of the invention in saving fuel, to observe what were the gains of the partnership under this stipulation. On one mine, that of Chasewater, in Cornwall, the proprietors compounded for £2,400 a-year, instead of paying the third of the fuel saved. That saving then must very considerably have exceeded £7,200 a-year. But there seemed some difficulty in carrying bargains of this kind into effect; and the genius of Watt, fertile in resources, immediately invented a small clock, called *the counter*, to be moved by the engine, and which accurately recorded every stroke it made. Payment being in proportion to the number of strokes, the clock was enclosed in a box under a double lock, and thus the working could be easily and securely ascertained.*

The first consequence of this grand invention, and the great saving of fuel it occasioned, was the renewed working of mines which had become unprofitable under the old plan. The next was the opening mines which Newcomen's engine could not drain at all. The steam-power, too, was no longer confined to draining mines. Various contrivances, for which Watt took out no less than four patents between 1781 and 1785, enabled him to communicate a rotatory motion from the piston, so that the engine could now work any machinery whatever; could spin cotton, cut iron and brass, stamp cloth, grind corn, print books, coin money, in short, could perform on any scale any kind of work in which human labour was either inefficient or expensive; and

* Such an engine could not be made and used secretly, and thus piracy was prevented. It is far otherwise with small pieces of mechanism, and still more difficult would be the protection of patent rights in mere methods, though to these the protection of the law should be extended.

while it was seen in one place pouring out rivers from the bowels of the earth with the arms of a giant, or cleaving rocks of granite formation, or clipping huge bars of stubborn iron into ribands, it was elsewhere to be found weaving or spinning like a quiet and industrious female, or turning a small lathe, or forming the fine wheels of a watch, or drawing out a thread too fine for sight; when the machine, instead of sawing the air aloft, and making the ground tremble around it, was placed quietly on a table like a candlestick or an inkstand. The latest use of the power, and the most important, is steam travelling by land and water. Watt himself early perceived this application of his engine; and in 1785 he took out a patent for moving carriages by steam, but he does not appear to have practically used his method. The attempts had been numerous, and from very early times, to propel vessels by steam. There seems reason to think that the paddle-wheel, the only addition to the steam-engine required for navigation, was known in ancient Egypt: it certainly was known to the Romans. In the middle of the sixteenth century a Spanish engineer exhibited a steam-vessel to Charles V. The Marquis of Worcester appears to have turned his attention to the subject from some parts of the work already cited, and so superciliously condemned by Hume; and Jonathan Hulls, in 1736, took out a patent for a kind of steam-tug. Various similar attempts were afterwards made, but with no success, and it was not until the steam-engine had been improved and had become generally used for all other purposes that it was applied to those of locomotion.

It is truly painful to reflect, that among the rewards which this great public benefactor was destined to reap for his invaluable services, was the lot of having to pass many years of his life in the unenviable situation of a party to suits at law and in equity, so numerous as might well have worn out the patience of any one

but him, whose unwearied perseverance had already toiled successfully against unnumbered difficulties of another kind. Such was, at that time, the patent law of this country; such, in some degree, it still is, though much improved. Inventive genius is placed between two dangers, and it can hardly escape the one without falling before the other.—If the invention is such that it requires some new demand to be created, or some novel taste to be introduced, before it can be much used, the period of the monopoly expires before any gain can be reaped. This is the more likely to happen if it comes in competition with things already made, and of which, at some expense, a considerable stock has been prepared, because a formidable interest is combined against the use of that new method which must displace the old, and render valueless this collected stock. I remember sitting on the trial of a patent for a new and admirable pianoforte; the only witness to its excellence being a sculptor of distinction who had once made such instruments, but had no longer any interest in crying down the invention; none of the trade could be trusted to give their opinion upon oath; all were, of course, in a combination against that improvement, which, if adopted, would render unsaleable their pianofortes already made.—If, on the other hand, the superiority of the invention is quite manifest, if the demand for it already exists, if no combination can prevent its coming into general use—for example, the making a new instrument for performing a known and necessary operation, or a new substance for supplying a general want already existing—then the inventor has to prepare himself for encountering piracy in all its forms; capitalists, who would be ashamed to violate the law in their own persons, encouraging men of no substance to infringe the patent, and omitting to pay the patentee's costs when these tools are defeated. My learned and ingenious kinsman, Dr. Forsyth, the inventor of the percussion lock, passed the fourteen

years of his patent right in courts of justice, and in every instance prevailed; but he found the pirates pennyless, the costs were to be paid, and he never gained one shilling by an invention which is, I believe, more universally used all over Europe than any other, except, perhaps, Argand's lamp. That invention was defeated in court, in consequence of the imperfect state of the law in those days, and of the absurd leaning of the Judges against all patentees; their Lordships displaying the utmost ingenuity in discovering flaws, and calling into action all the resources of legal astuteness in grinding, as they went on, new law for the defeat of the inventor. Of this, one instance only needs be given. If a specification contained ten good matters or processes, and by oversight one was either not original, or in any other respect did not answer the description given,* the courts held the patent wholly void, and not merely void for the erroneously described part, upon the subtle and senseless ground that the Crown had been deceived in the grant.

Mr. Watt had to struggle against this state of the law as well as against the shameless frauds, the conspiracies of dishonest, unprincipled men. During seven years and upwards he was condemned to lead the life of litigation; during seven years his genius was withdrawn from his own pursuits to become what he, no doubt, had, unfortunately for society, full time to make himself, an accomplished and learned lawyer; and it was not till five and thirty years after his invention had been made, that he was finally freed by a decision of the Court of King's Bench, in 1799, from a durance which lasted all the term of his patent, after all interest in the subject had expired by efflux of time. It was proved before a committee of the House of Commons in 1829, that had his statutory

* Turner v. Winter, 6 T. R.; Rex v. Fuller, 3 B. and A. My Acts of 1835 and 1840 have in great part remedied these sad defects in the law; others still remain.

term in the patent only been secured to him, he would have been a great loser by the invention; and that for some years after the Act of Parliament had extended the time, he still was out of pocket: consequently it follows, that had he never taken a patent at all, but trusted entirely to the preference which his being the inventor would have given him in the market, as a maker of steam-apparatus, that is, had he taken only this indirect benefit instead of the direct gains of the monopoly, he would have been better off in a pecuniary point of view than he was by means of the grant of the patent and its Parliamentary extension. The Act which I introduced in 1835, grounded mainly upon that evidence, has removed some of the greatest defects in the law; and it has enabled, when coupled with the subsequent Act of last Session, an inventor to obtain, at a very inconsiderable cost, an extension for any additional period, not exceeding the duration of the original patent.* The expenses of obtaining patents, and especially the grievous burden of having to take out one for each of the three kingdoms, are the principal parts of the grievance which remain to be redressed.

Notwithstanding the serious drawbacks upon his gains which Watt thus experienced, he was, on the whole, successful in respect of profit, realizing an ample fortune, but which all men wished had been greater, and which, under a more just law, would have been thrice as great.

We have been contemplating the great achievement of Watt, but it would be a mistake to suppose that the

* The course which a patentee ought to pursue if there be no opposition to his claim of extension, is to employ no solicitor and no counsel, but to appear in person before the Judicial Committee, as my gallant and truly ingenious friend Lord Dundonald (better known as Lord Cochrane) lately did. Their lordships will always favour such a course, the rather as they thus obtain the advantage of hearing the explanations required from the person best able to give them. In opposed cases professional aid is requisite.

steam-engine is the only monument of his scientific genius or his inventive skill. He was the author of the machine in general use for copying letters; of the method extensively used for heating buildings and hot-houses by steam; and of an ingenious mechanism for multiplying copies of busts and other sculptures; but he was also, without any doubt, the person who first discovered the composition of water. At this most important truth he arrived by a profound examination of all the experiments which had been made by Warltire, by Macquer, and especially by Priestley, upon the combustion of hydrogen and oxygen gases, then called inflammable, and vital or dephlogisticated airs. No former reasoner had come even near the true theory of the phenomena observed in those experiments. All had assumed that water was a simple or elementary body; that it was contained in the airs burnt together, and was precipitated by their explosion. He, on the contrary, showed that it was formed by the union of the two gases, and their parting with the latent heat which had held them in the elastic or gaseous state, but which being withdrawn by their union, left them in a state of liquid or aqueous fluidity.

As early as 1782, his attention had been closely directed to the experiments in which air is produced from water, and especially to those upon the combustion of inflammable air. In December of that year he had matured his theory, for we find him then announcing to De Luc his discovery, that "one element must be dismissed from the list;" water being, according to his doctrine (stated more explicitly to Dr. Black, April 1783,) "composed of dephlogisticated and inflammable airs deprived of a portion of their latent heat." To his whole correspondence with that great philosopher, with Smeaton, with Priestley, De Luc, and others, I have had access, and no trace is to be found in it that either he or they had even entertained the least suspicion of the same thing having before oc-

curred to any one else.* It is to be noted, too, that in 1784 Mr. Cavendish, after his celebrated experiment, had not attained by any means so clear a notion of the true doctrine as Mr. Watt explains in those previous letters.† I examined minutely the whole of this subject eight years ago, at the request of my distinguished colleague M. Arago, then engaged in preparing his 'Eloge' of Mr. Watt, who had also been our fellow-member of the Institute. The reader will find my statement of the evidence annexed to this account. But I cannot easily suppose that M. Arago ever intended, and I know that I never myself intended, to insinuate in the slightest degree a suspicion of Mr. Cavendish's having borrowed from Mr. Watt. He had, in all probability, been led to the same conclusion by his own researches, ignorant of Mr. Watt's speculations, which were a little earlier in point of time, just as Priestley when claiming, and justly claiming, the important discovery of oxygen (called by him, in accordance with the doctrine of Stahl, "dephlogisticated air"), never denied that Scheele also made the same discovery, (calling it "empyreal air"), without being aware of another having preceded him. Priestley, of course, treated the discreditable proceedings of Lavoisier in respect to this gas very differently, and so must all impartial men.

It must on no account be supposed that Watt cannot be considered as having discovered the composition of water, merely because he made no new experiments of particular moment, like Cavendish, to ascertain that capital point. No one refuses to Newton the discovery of gravitation as the controlling and directing power of the solar system; and yet he made not one of those observations upon which his theory rests; nay, he threw it aside for sixteen years when the erroneous

* Letters to Gilbert Hamilton of Glasgow, Fry of Bristol, Smeaton, De Luc—all dated March and April, 1783.

† See Life of Cavendish for further particulars and explanations.

notion of a degree being only sixty miles appeared by its consequences to disprove his proposition, and instead of making any further experiments himself, waited until Picard's more accurate measurement became known to him accidentally in 1682, and enabled him to demonstrate his doctrine. In like manner, Lavoisier, who discovered no gas, and made no original experiments of the least value in pneumatic chemistry, is universally admitted to have discovered the true theory of combustion and calcination, by reasoning on the facts which others had ascertained. Watt's happy inference from the facts discovered by Warltire and Priestley was just as much entitled, and for the same reasons, to be regarded as the discovery of the composition of water.

The latter years of Mr. Watt's useful and honourable life were passed in the bosom of his family and the society of his friends, although he ever gave the due attention to the extensive concerns of the house in which he was the principal partner. He had been married as early as 1764 to Miss Miller, his cousin, and had by her a daughter who predeceased him, and a son, James, who still survives, inheriting the scientific tastes, the extensive knowledge, the masculine understanding, and the scrupulous integrity of his father. With the late Mr. Robinson Boulton and Mr. Gregory Watt, he was admitted into the partnership, the concerns of which he has extended, and, for the last quarter of a century, almost exclusively conducted. By his second wife, Miss Macgregor, whom Mr. Watt married 1776, he had one son, Gregory, who unfortunately died in October, 1804, at the age of twenty-seven, after giving an earnest of brilliant talents and accomplishments. This loss was, no doubt, a severe blow to his family, and the father shared fully in their sorrow. But he bore it like a man: and I feel great satisfaction in correcting an error into which my illustrious friend and colleague M. Arago has fallen through

misinformation, when he represents Mr. Watt's spirit as so entirely broken by the misfortune that he "preserved an almost total silence during the latter years of his life." The fact is, that he survived his son's death nearly fifteen years, and never was more cheerful or enjoyed the pleasures of society more heartily than during this period. I can speak on the point with absolute certainty, for my own acquaintance with him commenced after my friend Gregory's decease. A few months after that event, he calmly and with his wonted acuteness discussed with me the composition of an epitaph to be inscribed on his son's tomb. The autumn and winter of 1805 he was a constant attendant at our Friday club, and in all our private circles, and was the life of them all. He has, moreover, left under his hand an account of the effect which the recent loss had produced upon his spirits, and a flat contradiction to the notion that it had depressed them. "I perhaps," he observes, "have said too much to you and Mr. Campbell on the state of my mind: I therefore think it necessary to say that I am not low-spirited, and were you here you would find me as cheerful in the company of my friends as usual; my feelings for the loss of poor Gregory are not passion, but a deep regret that such was his and my lot." He then expresses his pious resignation to the will of "the Disposer of events." It is true, he adds, that he had lost one stimulus to exertion, and with it his relish for his usual avocations, but he looks to time for a remedy, and adds, "meanwhile, I do not neglect the means of amusement which are within my power." This letter was written in January 1805, only a few weeks after the loss of his son. In another letter written in April to the same gentleman, his cousin, Mr. Muirhead, great uncle of the able and learned translator of M. Arago's 'Eloge,' after expressing his confident hopes that Gregory had changed this mortal state for a far happier existence, he says, as if anxious to avoid all suspicion

of his giving way to excessive sorrow, "You are not to conceive that we give way to grief: on the contrary, you will find us as cheerful as we ought to be, and as much disposed to enjoy the friends we have left as ever. But we should approach to brutes if we had no regrets." In this letter he quotes the beautiful lines of Catullus, "Nunc it per iter tenebricosum," &c.

To this evidence at the period of his son's death let me add the testimony of Lord Jeffrey, who knew him well, and who brings down the account to the latest years of his life. "His health, which was delicate from his youth upwards, seemed to become firmer as he advanced in years; and he possessed, up almost to the last moments of his existence, not only the full command of his extraordinary intellect, but all the alacrity of spirit and the social gaiety which had illumined his happiest days. His friends in this part of the country (Edinburgh) never saw him more full of intellectual vigour and colloquial animation, never more delightful or more instructive, than in his last visit in autumn 1817." It was after this period that he invented the machine for copying sculpture. He distributed among his friends some specimens of its performances, jocularly calling them "the productions of a young artist just entering into his eighty-third year."

In the summer of the following year, 1819, I saw him for the last time, and did not observe any change in his conversation or in his manner; but I understand that he suffered some inconvenience through the summer; though, until a few weeks before his death, he was not seriously indisposed. He soon became aware of the event which was approaching, and he seemed only anxious to impress upon his sorrowing family the circumstances calculated to minister consolation under the change which must soon take place. He expressed his sincere gratitude to Divine Providence for the blessings which he had been permitted to enjoy, for his length of days, his exemption from

the infirmities of age, the calm and cheerful evening of his life passed after the useful labours of its day had closed. He died on the 25th of August, 1819, in his eighty-fourth year. His remains lie buried in Handsworth church, near his residence of Heathfield, and a statue, the work of Chantrey, is there erected to his memory by his son; and the same filial piety has presented a statue to the College of Glasgow, in grateful recollection of early patronage. But a truly noble monument is raised to him in Westminster Abbey, by the genius of Chantrey, at the expense of the sovereign and of many peers and distinguished commoners, who held a meeting in honour of this illustrious man and great public benefactor. The ministers of the crown, and the chiefs of the opposition in either House of Parliament, the most eminent men of science, the most distinguished cultivators of the arts, assembled with this view, and the account of their proceedings was made public in an authentic form. The prime minister, Lord Liverpool, presided; and it was none of the least remarkable passages of that day, that his successor, the present premier, was anxious to declare the obligation under which he lay to the genius of him they were commemorating, the fortunes of his family being reared by manufacturing industry, founded upon the happy inventions of Arkwright and Watt. It has ever been reckoned by me one of the chief honours of my life, that I was called upon to pen the inscription upon the noble monument thus nobly reared.

The chisel of Chantrey, whose greatest work this certainly is, has admirably presented the features of the countenance at once deeply meditative and calmly placid, but betokening power rather than delicacy and refinement. The civilized world is filled with imperishable records of his genius, and the grateful recollection of the whole species embalms his memory. But for this, the author of the epitaph might well feel how inadequately his feeble pen had performed its

office in attempting to pourtray such excellence: how much more inadequately when its lines are traced in most disadvantageous contrast with the signal success of the sculptor! He who has ever made the attempt to write with a chisel in our language, little lapidary as it certainly is, will comprehend the extraordinary difficulties of the task, and will show mercy to the failure:—

NOT TO PERPETUATE A NAME
WHICH MUST ENDURE WHILE THE PEACEFUL ARTS FLOURISH
BUT TO SHEW
THAT MANKIND HAVE LEARNED TO HONOUR THOSE
WHO BEST DESERVE THEIR GRATITUDE
THE KING
HIS MINISTERS AND MANY OF THE NOBLES
AND COMMONERS OF THE REALM
RAISED THIS MONUMENT TO
JAMES WATT
WHO DIRECTING THE FORCE OF AN ORIGINAL GENIUS
EARLY EXERCISED IN PHILOSOPHIC RESEARCH
TO THE IMPROVEMENT OF
THE STEAM ENGINE
ENLARGED THE RESOURCES OF HIS COUNTRY
INCREASED THE POWER OF MAN
AND ROSE TO AN EMINENT PLACE
AMONG THE MOST ILLUSTRIOUS FOLLOWERS OF SCIENCE
AND THE REAL BENEFACTORS OF THE WORLD
BORN AT GREENOCK MDCCXXXVI
DIED AT HEATHFIELD IN STAFFORDSHIRE MDCCCXIX.

We have been considering this eminent person as yet only in his public capacity, as a benefactor of mankind by his fertile genius and indomitable perseverance; and the best portraiture of his intellectual character was to be found in the description of his attainments. It is, however, proper to survey him also in private life. He was unexceptionable in all its relations; and as his activity was unmeasured, and his taste anything rather than fastidious, he both was master of every variety of knowledge, and was tolerant of discussion on subjects of very subordinate importance compared with those on which he most excelled. Not only all the sciences from the mathematics and

astronomy, down to botany, received his diligent attention, but he was tolerably read in the lighter kinds of literature, delighting in poetry and other works of fiction, full of the stores of ancient literature, and readily giving himself up to the critical disquisitions of commentators, and to discussions on the fancies of etymology. His manners were most attractive from their perfect nature and simplicity. His conversation was rich in the measure which such stores and such easy taste might lead us to expect, and it astonished all listeners with its admirable precision, with the extraordinary memory it displayed, with the distinctness it seemed to have, as if his mind had separate niches for keeping each particular, and with its complete rejection of all worthless and superfluous matter, as if the same mind had some fine machine for acting like a fan, casting off the chaff and the husk. But it had besides a peculiar charm from the pleasure he took in conveying information where he was peculiarly able to give it, and in joining with entire candour whatever discussion happened to arise. Even upon matters on which he was entitled to pronounce with absolute authority, he never laid down the law, but spoke like any other partaker of the conversation. You might observe him, however, with his pencil in his hand, ready to prove what might require explanation, and he was an adventurous disputant who would not rather see his intellect play in illustrations than descend with demonstrative force. He was ever in pursuit of truth or the gratification of a rational curiosity, and this attempered as well as guided his talk. If he seemed occasionally to be moved beyond the interest thus excited, it was when he perceived any thing uncandid or unfair, or, above all, indirect and dishonest. The attempts of one man to appropriate another's inventive merit were the things that most roused his indignation; for, regarding discovery and invention as the most precious of all property, he could not bear the sight of its violation,

and would stop minutely and curiously to ascertain the relative shares of different individuals, when any doubt was raised upon the distribution. His conversation was withal spirited and lively—it was easy and concise, and without the least of a lecturing formality. His voice was deep and low, and if somewhat monotonous, it yet seemed in harmony with the weight and the beauty of his discourse, through which, however, there also ran a current of a lighter kind; for he was mirthful, temperately jocular, nor could anything to more advantage set off the living anecdotes of men and things, with which the graver texture of his talk was interwoven, than his sly and quiet humour, both of mind and of look, in recounting them. No one who had the happiness of knowing him, no member, more especially, of the club in Edinburgh which he frequented as often as he visited that capital, can ever forget the zest which his society derived from the mixture of such various matters as those to which I have referred; and one of its most distinguished founders* has justly said, that in no other person was there ever observed so "fine an expression of reposing strength and uninterrupted self-possession as marked his whole manner."

* Lord Jeffrey. The club was called from the day, Friday, on which it met at supper, after the business of the week was over, and the half-holiday of Saturday only lightly hanging over the heads of the lawyers, who chiefly composed it. Mr. Watt was an honorary member. He had for his colleagues no less distinguished men than Professor Playfair, Sir Walter Scott, Lord Corehouse, Mr. Horner, Mr. Elmsley, Sir W. Drummond, and several who still survive and fill exalted places in the state.

APPENDIX.

HISTORICAL NOTE ON THE DISCOVERY OF THE THEORY OF THE COMPOSITION OF WATER.

THERE can be no doubt whatever, that the experiment of Mr. Warltire, related in Dr. Priestley's fifth volume,* gave rise to this inquiry, at least in England; Mr. Cavendish expressly refers to it, as having set him upon making his experiments.—(Phil. Trans. 1784, p. 126.) The experiment of Mr. Warltire consisted in firing, by electricity, a mixture of inflammable and common air in a close vessel, and two things were said to be observed: *first*, a sensible loss of weight; *second*, a dewy deposit on the sides of the vessel.

Mr. Watt, in a note to p. 332 of his paper, Phil. Trans. 1784, inadvertently states, that the dewy deposit was first observed by Mr. Cavendish; but Mr. Cavendish himself, p. 127, expressly states Mr. Warltire to have observed it, and cites Dr. Priestley's fifth volume.

* Mr. Warltire's letter is dated Birmingham, 18th April, 1781, and was published by Dr. Priestley in the Appendix to the seventh volume of his 'Experiments and Observations relating to various branches of Natural Philosophy; with a continuation of the Observations on Air,'—forming, in fact, the fifth volume of his 'Experiments and Observations on different kinds of Air;' printed at Birmingham in 1781.

Mr. Warltire's first experiments were made in a copper ball or flask, which held three wine pints, the weight 14 ounces; and his object was to determine "whether heat is heavy or not." After stating his mode of mixing the airs, and of adjusting the balance, he says, he "always accurately balanced the flask of common air, then found the difference of weight after the inflammable air was introduced, that he might be certain he had confined the proper proportion of each. The electric spark having passed through them, the flask became hot, and was cooled by exposing it to the common air of the room: it was then hung up again to the balance, and a loss of weight was always found, but not constantly the same; upon an average it was two grains."

He goes on to say, "I have fired air in glass vessels since I saw you (Dr. Priestley) venture to do it, and I have observed, *as you did*, that, though the glass was clean and dry before, yet, after firing the air, it became dewy, and was lined with a sooty substance."

It seems evident that neither Mr. Warltire nor Dr. Priestley attributed the dew to anything else than a mechanical deposit of the moisture suspended in common air.—[*Note by Mr. James Watt.*]

Mr. Cavendish himself could find no loss of weight, and he says that Dr. Priestley had also tried the experiment, and found none.* But Mr. Cavendish found there was always a dewy deposit, without any sooty matter. The result of many trials was, that common air and inflammable air being burnt together, in the proportion of 1000 measures of the former to 423 of the latter, "about one-fifth of the common air, and nearly all the inflammable air, lose their elasticity, and *are condensed into the dew* which lines the glass." He examined the dew, and found it to be pure water. He therefore concludes, that "almost all the inflammable air, and about one-sixth of the common air, are turned into pure water."

Mr. Cavendish then burned, in the same way, dephlogisticated and inflammable airs (oxygen and hydrogen gases), and the deposit was always more or less acidulous, accordingly as the air burnt with the inflammable air was more or less phlogisticated. The acid was found to be nitrous. Mr. Cavendish states, that "almost the whole of the inflammable and dephlogisticated air is *converted into pure water*;" and, again, that "if these airs could be obtained perfectly pure, the whole would be condensed." And he accounts for common air and inflammable air, when burnt together, not producing acid, by supposing that the heat produced is not sufficient. He then says that these experiments, with the exception of what relates to the acid, were made in the summer of 1781, and mentioned to Dr. Priestley; and adds, that "a friend of his (Mr. Cavendish's), last summer (that is 1783), gave some account of them to M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of its phlogiston; but, at that time, so far was M. Lavoisier from thinking any such opinion warranted, that till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water." The friend is known to have been Dr., afterwards Sir Charles Blagden; and it is a remarkable circumstance, that this passage of Mr. Cavendish's paper

* Mr. Cavendish's note, p. 127, would seem to imply this; but I have not found in any of Dr. Priestley's papers that he has said so.—[*Note by Mr. James Watt.*]

appears not to have been in it when originally presented to the Royal Society; for the paper is apparently in Mr. Cavendish's hand, and the paragraph, p. 134, 135, is not found in it, but is added to it, and directed to be inserted in that place. It is, moreover, not in Mr. Cavendish's hand, but in Sir Charles Blagden's; and, indeed, the latter must have given him the information as to M. Lavoisier, with whom it is not said that Mr. Cavendish had any correspondence. The paper itself was read 15th January, 1784. The volume was published about six months afterwards.

M. Lavoisier's memoir (in the Mém. de l'Académie des Sciences for 1781) had been read partly in November and December 1783, and additions were afterwards made to it. It was published in 1784. It contained M. Lavoisier's account of his experiments in June 1783, at which, he says, Sir Charles Blagden was present; and it states that he told M. Lavoisier of Mr. Cavendish having "already burnt inflammable air in close vessels, and obtained a very sensible quantity of water." But he, M. Lavoisier, says nothing of Sir Charles Blagden having also mentioned Mr. Cavendish's conclusion from the experiment. He expressly states, that the weight of the water was equal to that of the two airs burnt, unless the heat and light which escape are ponderable, which he holds them not to be His account, therefore, is not reconcilable with Sir Charles Blagden's, and the latter was most probably written as a contradiction of it, after Mr. Cavendish's paper had been read, and when the Mémoires of the Académie were received in this country. These Mémoires were published in 1784, and could not, certainly, have arrived when Mr. Cavendish's paper was written, nor when it was read to the Royal Society

But it is further to be remarked, that this passage of Mr. Cavendish's paper in Sir Charles Blagden's handwriting, only mentions the experiments having been communicated to Dr. Priestley; they were made, says the passage, in 1781, and communicated to Dr. Priestley; it is not said when, nor is it said that "the conclusions drawn from them," and which Sir Charles Blagden says he communicated to M. Lavoisier in summer 1783, were ever communicated to Dr. Priestley; and Dr. Priestley, in his paper

(referred to in Mr. Cavendish's), which was read June 1783, and written before April of that year, says nothing of Mr. Cavendish's theory, though he mentions his experiment.

Several propositions then are proved by this statement.

First, That Mr. Cavendish, in his paper, read 15th January, 1784, relates the capital experiment of burning oxygen and hydrogen gases in a close vessel, and finding pure water to be the produce of the combustion.

Secondly, That, in the same paper, he drew from this experiment the conclusion that the two gases were converted or turned into water.

Thirdly, That Sir Charles Blagden inserted in the same paper, with Mr. Cavendish's consent, a statement that the experiment had first been made by Mr. Cavendish in summer 1781, and mentioned to Dr. Priestley, though it is not said when, nor is it said that any conclusion was mentioned to Dr. Priestley, nor is it said at what time Mr. Cavendish first drew that conclusion. A most material omission.

Fourthly, That in that addition made to the paper by Sir Charles Blagden, the conclusion of Mr. Cavendish is stated to be, that oxygen gas is water deprived of phlogiston; this addition having been made after M. Lavoisier's memoir arrived in England.

It may further be observed, that in another addition to the paper which is also in Sir C. Blagden's handwriting, and which was certainly made after M. Lavoisier's memoir had arrived, Mr. Cavendish for the first time distinctly states, as upon M. Lavoisier's hypothesis, that water consisted of hydrogen united to oxygen gas. There is no substantial difference, perhaps, between this and the conclusion stated to have been drawn by Mr. Cavendish himself, that oxygen gas is water deprived of phlogiston, supposing phlogiston to be synonymous with hydrogen; but the former proposition is certainly the more distinct and unequivocal of the two: and it is to be observed that Mr. Cavendish, in the original part of the paper, *i. e.* the part read January 1784, before the arrival of Lavoisier's, considers it more just to hold inflammable air to be phlogisticated water than pure phlogiston (p. 140.)

We are now to see what Mr. Watt did; and the dates

here become very material. It appears that he wrote a letter to Dr. Priestley on 26th April, 1783, in which he reasons on the experiment of burning the two gases in a close vessel, and draws the conclusion, "that water is composed of dephlogisticated air and phlogiston, deprived of part of their latent heat."* The letter was received by Dr. Priestley and delivered to Sir Joseph Banks, with a request that it might be read to the Royal Society; but Mr. Watt afterwards desired this to be delayed, in order that he might examine some new experiments of Dr. Priestley, so that it was not read until the 22d April, 1784. In the interval between the delivery of this letter to Dr. Priestley, and the reading of it, Mr. Watt had addressed another letter to M. De Luc, dated 26th November, 1783,†

* It may with certainty be concluded from Mr. Watt's private and unpublished letters, of which the copies taken by his copying-machine, then recently invented, are preserved, that his theory of the composition of water was already formed in December, 1782, and probably much earlier. Dr. Priestley, in his paper of 21st April, 1783, p. 416, states, that Mr. Watt, prior to his (the Doctor's) experiments, had entertained the idea of the possibility of the conversion of water or steam into permanent air. And Mr. Watt himself, in his paper, Phil. Trans., p. 335, asserts, that for many years he had entertained the opinion that air was a modification of water, and he enters at some length into the facts and reasoning upon which that deduction was founded.—[*Note by Mr. James Watt.*]

† The letter was addressed to M. J. A. De Luc, the well-known Genevese philosopher, then a Fellow of the Royal Society, and Reader to Queen Charlotte. He was the friend of Mr. Watt, who did not then belong to the Society. M. De Luc, following the motions of the Court, was not always in London, and seldom attended the meetings of the Royal Society. He was not present when Mr. Cavendish's paper of 15th January, 1784, was read; but, hearing of it from Dr. Blagden, he obtained a loan of it from Mr. Cavendish, and writes to Mr. Watt on the 1st March following, to apprise him of it, adding that he has perused it, and promising an analysis. In the postscript he states, "In short, they expound and prove your system, word for word, and say nothing of you." The promised analysis is given in another letter of the 4th of the same month. Mr. Watt replies on the 6th, with all the feelings which a conviction he had been ill-treated was calculated to inspire, and makes use of those vivid expressions which M. Arago has quoted; he states his intention of being in London in the ensuing week, and his opinion, that the reading of his letter to the Royal Society will be the proper step to be taken. He accordingly went there, waited upon the President of the Royal Society, Sir Joseph Banks, was received with all the courtesy and just feeling which distinguished that most honourable man; and it was settled that

with many further observations and reasonings, but almost the whole of the original letter is preserved in this, and is distinguished by inverted commas. One of the passages thus marked is that which has the important conclusion above mentioned; and that letter is stated, in the subsequent one, to have been communicated to several members of the Royal Society at the time of its reaching Dr. Priestley, viz. April, 1783.

In Mr. Cavendish's paper as at first read, no allusion is to be found to Mr. Watt's theory; but in an addition made in Sir C. Blagden's own hand, after Mr. Watt's paper had been read, there is a reference to that theory (Phil. Trans. 1784, p. 140), and Mr. Cavendish's reasons are given for not encumbering his theory with that part of Mr. Watt's which regards the evolution of latent heat. It is thus left somewhat doubtful, whether Mr. Cavendish had ever seen the letter of April 1783, or whether he had seen only the paper (of 26th November, 1783) of which that letter formed a part, and which was read 29th April, 1784. That the first letter was for some time (two months, as appears from the papers of Mr. Watt) in the hands of Sir Joseph Banks and other members of the Society, during the preceding spring, is certain, from the statements in the note to p. 330; and that Sir Charles Blagden, the Secretary, should not have seen it, seems impossible; for Sir Joseph Banks must have delivered it to him at the time when it was intended to be read at one of the Society's meetings (Phil. Trans., p. 330, Note), and, as the letter itself remains among the Society's Records, in the same volume with the paper into which the greater part of it was introduced, it must have been in the custody of Sir C. Blagden. It is equally difficult to suppose, that the person who wrote the remarkable passage already referred to, respecting Mr. Cavendish's conclusions having been communicated to M. Lavoisier in the summer of 1783 (that is, in June), should not have mentioned to Mr. Cavendish that Mr. Watt had drawn the same conclusion in the spring of 1783 (that is,

both the letter to Dr. Priestley of 26th April, 1783, and that to M. De Luc of 26th November, 1783, should be successively read. The former was done on the 22d, and the latter on the 29th April, 1784.—[*Note by Mr. James Watt.*]

in April at the latest.) For the conclusions are identical, with the single difference, that Mr. Cavendish calls dephlogisticated air, water deprived of its phlogiston, and Mr. Watt says that water is composed of dephlogisticated air and phlogiston.

We may remark, there is the same uncertainty or vagueness introduced into Mr. Watt's theory, which we before observed in Mr. Cavendish's, by the use of the term Phlogiston, without exactly defining it. Mr. Cavendish leaves it uncertain, whether or not he meant by phlogiston simply inflammable air, and he inclines rather to call inflammable air, water united to phlogiston. Mr. Watt says expressly, even in his later paper (of November, 1783), and in a passage not to be found in the letter of April, 1783, that he thinks that inflammable air contains a small quantity of water, and much elementary heat. It must be admitted that such expressions as these on the part of both of those great men, betoken a certain hesitation respecting the theory of the composition of water. If they had ever formed to themselves the idea that water is a compound of the two gases deprived of their latent heat,—that is, of the two gases,—with the same distinctiveness which marks M. Lavoisier's statement of the theory, such obscurity and uncertainty would have been avoided.

Several further propositions may now be stated, as the result of the facts regarding Mr. Watt.

First, That there is no evidence of any person having reduced the theory of composition to writing, in a shape which now remains, so early as Mr. Watt.

Secondly, That he states the theory, both in April and November, 1783, in language somewhat more distinctly referring to composition than Mr. Cavendish does in 1784, and that his reference to the evolution of latent heat renders it more distinct than Mr. Cavendish's.

Thirdly, That there is no proof, nor even any assertion, of Mr. Cavendish's theory (what Sir C. Blagden calls his conclusion) having been communicated to Dr. Priestley before Mr. Watt stated his theory in 1783, still less of Mr. Watt having heard of it, while his whole letter shows that he never had been aware of it, either from Dr. Priestley, or from any other quarter.

Fourthly, That Mr. Watt's theory was well known among the members of the Society, some months before Mr. Cavendish's statement appears to have been reduced into writing, and eight months before it was presented to the Society. We may indeed go further, and affirm, as another deduction from the facts and dates, that as far as the evidence goes, there is proof of Mr. Watt having first drawn the conclusion, at least that no proof exists of any one having drawn it so early as he is proved to have done.

Lastly, That a reluctance to give up the doctrine of phlogiston, a kind of timidity on the score of that long-established and deeply rooted opinion, prevented both Mr. Watt and Mr. Cavendish from doing full justice to their own theory; while M. Lavoisier, who had entirely shaken off these trammels, first presented the new doctrine in its entire perfection and consistency.

All three may have made the important step nearly at the same time, and unknown to each other; the step, namely, of concluding from the experiment, that the two gases entered into combination, and that water was the result; for this, with more or less of distinctness, is the inference which all three drew.

But there is the statement of Sir Charles Blagden, to show that M. Lavoisier had heard of Mr. Cavendish's drawing this inference before his (M. Lavoisier's) capital experiment was made; and it appears that M. Lavoisier, after Sir C. Blagden's statement had been embodied in Mr. Cavendish's paper and made public, never gave any contradiction to it in any of his subsequent memoirs which are to be found in the Mémoires de l'Académie, though his own account of that experiment, and of what then passed, is inconsistent with Sir Charles Blagden's statement.

But there is not any assertion at all, even from Sir C. Blagden, zealous for Mr. Cavendish's priority as he was, that Mr. Watt had ever heard of Mr. Cavendish's theory before he formed his own.

Whether or not Mr. Cavendish had heard of Mr. Watt's theory previous to drawing his conclusions, appears more doubtful. The supposition that he had so heard, rests on the improbability of his (Sir Charles Blagden's) and many others knowing what Mr. Watt had done, and not com-

municating it to Mr. Cavendish, and on the omission of any assertion in Mr. Cavendish's paper, even in the part written by Sir C. Blagden with the view of claiming priority as against M. Lavoisier, that Mr. Cavendish had drawn his conclusion before April 1783, although in one of the additions to that paper reference is made to Mr. Watt's theory.

As great obscurity hangs over the material question at what time Mr. Cavendish first drew the conclusion from his experiment, it may be as well to examine what that great man's habit was in communicating his discoveries to the Royal Society.

A Committee of the Royal Society, with Mr. Gilpin the clerk, made a series of experiments on the formation of nitrous acid, under Mr. Cavendish's direction, and to satisfy those who had doubted his theory of its composition, first given accidentally in the paper of January 1784, and afterwards more fully in another paper, June 1785. Those experiments occupied from the 6th December, 1787, to 19th March, 1788, and Mr. Cavendish's paper upon them was read 17th April, 1788. It was, therefore, written and printed within a month of the experiments being concluded.

Mr. Kirwan answered Mr. Cavendish's paper (of 15th January, 1784) on water, in one which was read 5th February, 1784, and Mr. Cavendish replied in a paper read 4th March, 1784.

Mr. Cavendish's experiments on the density of the earth were made from the 5th August, 1797, to the 27th May, 1798. The paper upon that subject was read 27th June, 1798.

The account of the eudiometer was communicated at apparently a greater interval; at least the only time mentioned in the account of the experiments is the latter half of 1781, and the paper was read January 1783. It is, however, probable, from the nature of the subject, that he made further trials during the year 1782.

That Mr. Watt formed his theory during the few months or weeks immediately preceding April, 1783, seems probable.* It is certain that he considered the theory as his

* That the idea existed in his mind previously, is proved by his declarations to Dr. Priestley, cited by the latter; by his own assertions,

own, and makes no reference to any previous communication from any one upon the subject, nor of having ever heard of Mr. Cavendish drawing the same conclusion.

The improbability must also be admitted to be extreme, of Sir Charles Blagden ever having heard of Mr. Cavendish's theory prior to the date of Mr. Watt's letter, and not mentioning that circumstance in the insertion which he made in Mr. Cavendish's paper.

It deserves to be farther mentioned, that Mr. Watt left the correction of the press, and every thing relating to the publishing of his paper, to Sir Charles Blagden. A letter remains from him to that effect, written to Sir Charles Blagden, and Mr. Watt never saw the paper until it was printed.

Since M. Arago's learned Eloge was published, with this paper as an Appendix, the Rev. W. Vernon Harcourt has entered into controversy with us both, or, I should rather say, with M. Arago, for he has kindly spared me; and while I express my obligations for this courtesy of my reverend, learned, and valued friend, I must express my unqualified admiration of his boldness in singling out for his antagonist my illustrious colleague, rather than the far weaker combatant against whom he might so much more safely have done battle. Whatever might have been his fate had he taken the more prudent course, I must fairly say (even without waiting until my fellow-champion seal our adversary's doom), that I have seldom seen any two parties more unequally matched, or any disputation in which the victory was so complete. The attack on M. Arago might have passed well enough at a popular meeting at Birmingham, before which it was spoken; but as a scientific inquirer, it would be a flattery running the risk of seeming ironical to weigh the reverend author against the most eminent philosopher of the day, although upon a question of evidence (which this really is, as well as a scientific discussion) I might be content to succumb before him. As a strange notion, however, seems to pervade this paper, that

p. 335 of his paper; and by the existing copies of his letters in December 1782.—[*Note by Mr. James Watt.*]

everything depends on the character of Mr. Cavendish, it may be as well to repeat the disclaimer already very distinctly made of all intention to cast the slightest doubt upon that great man's perfect good faith in the whole affair; I never having supposed that he borrowed from Mr. Watt, though M. Arago, Professor Robison,* and Sir H. Davy, as well as myself, have always been convinced that Mr. Watt had, unknown to him, anticipated his great discovery. It is also said by Mr. Harcourt, that the late Dr. Henry having examined Mr. Watt's manuscripts decided against his priority. I have Dr. H.'s letter before me of June 1820, stating most clearly, most fully, and most directly, the reverse, and deciding in Mr. Watt's favour. I must add, having read the full publication with fac-similes, Mr. Harcourt has now clearly proved one thing, and it is really of some importance. He has made it appear that in all Mr. Cavendish's diaries and notes of his experiments, not an intimation occurs of the composition of water having been inferred by him from those experiments earlier than Mr. Watt's paper of spring, 1783.

* Encyc. Brit., vol. xviii., p. 808. This able and learned article enters at length into the proofs of Mr. Watt's claims, and it was published in 1797, thirteen years before Mr. Cavendish's death.

PRIESTLEY.

MENTION has already been more than once made of Dr. Priestley; and certainly history would imperfectly perform its office of recording the progress of natural knowledge should it pass over his important discoveries without the large share of attention and of praise which they are well entitled to claim. In turning, however, to recount the events of his life, we make a somewhat painful transition from contemplating the philosophic character in its perfection, to follow the course of one who united in his own person the part of the experimental inquirer after physical truth with that of the angry polemic and the fiery politician, leading sometimes the life of a sage, though never perhaps free from rooted and perverted prejudice—sometimes that of a zealot against received creeds and established institutions, and in consequence of his intemperance, alternately the exciter and the victim of persecution. Nevertheless, the services which he rendered in the former and better capacity, ought to be held in grateful remembrance by the cultivators of physical science. Nor are we to suppose that even in his polemical capacity he was not in pursuit of truth. He may have had a tendency to oppose established opinions; a disposition which led him, as he says himself, at the age of twenty "to embrace what is generally called the heterodox side of every question,"* just as he had a disposition pertinaciously to keep by the received and erroneous chemical theory; but if he thought for him-

* Works.—Memoirs, vol. i., part i., p. 25.

self, and followed the bent of his convictions, we have no right to doubt his conscientious motives, the more especially as his heterodox dogmas, always manfully avowed, never brought him anything but vexation and positive injury in his temporal concerns. The pertinacity with which he defended to the end of his days the chemical doctrine of Phlogiston, and the equal zeal with which he attacked the theological tenets of original sin and the atonement, alike proceeded from sincere conviction, and no one has a right to blame him for either of these opinions, even if it be quite clear that he was wrong in both.

Joseph Priestley was the son of a cloth dresser at Birstal-Fieldhead, near Leeds, and was born there, 13th of March (old style), 1733. His family appear to have been in humble circumstances; and he was taken off their hands after the death of his mother by his paternal aunt, with whom he went to live when nine years old, and who sent him to a free school at Batley, in the neighbourhood. There he learnt something of Greek and Latin, and a dissenting minister taught him a little Hebrew in the vacation of the grammar-school. To this he added some knowledge of other Eastern languages connected with Biblical literature; he made a considerable progress in Syriac and Chaldean, and began to learn Arabic; he also had a little instruction in the mathematics from a teacher who had been educated under Maclaurin, at Edinburgh. But in this science he made very little proficiency.* Indeed his whole education was exceedingly imperfect, and excepting in Hebrew and in Greek he never afterwards improved it by any systematic course of study; but in both these languages he became well versed, and he

* This is manifest from several parts of his writings, although he in one passage of his correspondence speaks of having once been very fond of the study; for in the same paper he speaks of Baron Maseres' work ('Scriptores Logarithmici') as if he had been the author, instead of the collector.—Mem. i., part ii., p. 490.

used always to read the Scriptures in the original tongues. Even in chemistry, the science which he best knew, and in which he made so important a figure, he was only half taught; and he himself acknowledged, after having failed to obtain a chemical lectureship, that he "never could have acquitted himself properly in it, never having given much attention to the common routine of the science, and knowing but little of the common processes."—"When I began my experiments," he says, "I knew very little of chemistry, and had, in a manner, no idea of the subject before I attended a course of lectures at an academy where I taught." So that he was not well-informed, and had never studied either the theoretical or the practical parts of it, but just got possession of such portions of the subject as occasionally came within the scope of the experiments he was making, and the doctrines he was discussing at the time. His whole writings, which are numberless, and without method, or system, or closeness, or indeed clearness, bear ample testimony to what we might expect would be the result of so very imperfect a foundation as his scanty and rambling education had laid. That education, however, far from redounding to his discredit, very greatly enhances the merit of the man. He presents one of the memorable examples of knowledge pursued, science cultivated, and even its bounds extended, by those whose circumstances made their exertions a continued struggle against difficulties which only virtue and genius like theirs could have overcome.

He went to study for some years at the dissenting academy founded by Mr. Coward, at Daventry, and since transferred to London, where it is in a kind of union, mutually beneficial, with the University College. Mr. Ashworth had succeeded the learned and pious Dr. Doddridge as its principal teacher, and under him Priestley remained till 1755. During the three years that he studied here, he and his intimate friends used

to make a point of reading, daily, ten pages of Greek, and every week one Greek play, a practice which they continued after they left the school, corresponding with each other on the subject of their studies. On quitting Daventry, having taken orders, he was appointed minister of a congregation at Needham Market, in Suffolk. He had been brought up by his father and aunt in the strictest Calvinistic principles, most of which he very soon from conviction abandoned; and so early did his spirit of free inquiry show itself, that having before he left his aunt's house desired to be admitted as a communicant at the chapel which she attended, he was rejected by the minister on his preparatory examination, in consequence of doubts expressed respecting original sin, and eternal damnation as its punishment. He describes the deep distress into which he was thrown by feeling that he was unable to experience due contrition and repentance for Adam's fault; and the rigid divine who tested the state of his mind on this point, withheld the sacred ordinances in consequence. At Needham his salary never exceeded thirty pounds; indeed it seldom amounted to so much, and he could only subsist by the aid which certain dissenting charities afforded to augment this poor stipend. His predecessor, Dr. Doddridge, had never received above thirty-five pounds a-year, and his board then (1723) only cost him ten pounds. Priestley's opinions proved distasteful to the congregation, who probably regarded the eternity of hell-torments as a peculiar privilege rudely invaded by him; and he removed in 1758 to Nantwich, in Cheshire, where he obtained some thirty pupils, beside teaching a few young ladies and acting as private tutor in an attorney's family. This increased his income, and enabled him, by means of the strictest frugality, to purchase a scanty apparatus; for he had now added a little natural philosophy to his favourite theological studies, the fruit of which had been already two works,

one of them against the atonement. I say a little natural philosophy; for he confesses that when nine years later he began to write his 'History of Electricity,' he was but imperfectly acquainted with the subject. It is a careless and superficial work, hastily written, as is his 'History of Vision;' and the original experiments afforded no new information of any value. In 1761 he removed to Warrington Academy, in which he succeeded Dr. Aikin as tutor in the belles lettres. On settling at Warrington he married the daughter of Mr. Wilkinson, a respectable iron master in Wales. She was an amiable woman, and endowed with great strength of mind, which was destined afterwards to be severely tried. By her he had several children, one of whom survived them both.

He appears to have chiefly devoted himself to theological studies, and hence the great disproportion which his Hebrew and Greek learning bears to his other acquirements. Metaphysical speculations, next to these, engaged his attention; and the influence produced on his mind, and even his conduct, by Dr. Hartley's celebrated work ('Observations on Man'), has been recorded by himself. "I hardly know," he says, "whether it more enlightens the understanding or improves the heart." He says he also had studied composition, and mainly by the help of writing poetry, of no merit, but according to him the best means of learning to write good prose. That his taste, however, was somewhat deficient in this respect, we may fairly affirm, when we find him pronouncing, many years after, a decided opinion that Belsham's 'History' is written in a better style than Robertson's or Hume's.* The universality of his attempts may be judged from his delivering at Warrington a course of lectures on anatomy. He sought relaxation from music, and learnt to play on the flute. He strongly recommends

* Mem. and Cor. 1796, vol i., part ii., p. 358.

this to students, especially, he says with some naïveté, such as have no fine ear, "for they will be the less annoyed by bad music."

As early as during his education at Daventry he had written a work which, however, was not published till twenty years later; it was the 'Institutes of Natural and Revealed Religion.' But having once begun to publish in 1761, his appeals to the press were incessant, and on almost every subject. A 'Theory of Language,' books on 'Oratory and Criticism,' on 'History and General Policy,' on the 'Constitution and Laws of England,' on 'Education,' a 'Chart of Biography,' a 'Chart of History;' these and others were all written while he resided at Warrington, from 1761 to 1769. How well he was qualified to write on oratory and on English law, we may easily conjecture, from the circumstance that he could never have heard any speaking save in the pulpits of meeting-houses, and in all probability had never seen a cause tried; but even if he had been present at debates and trials, it is difficult to imagine anything more adventurous than the tutor of an academy, afflicted with an incurable stutter, and who devoted his time to teaching and to theology, promulgating rules of eloquence and of jurisprudence to the senators and lawyers of his country. That we may come without interruption to his really useful studies, it may be well here to take notice of his other controversial writings. In consequence of a disagreement with the Warrington trustees in 1767 he removed to Leeds, where he became minister of the Mill-Hill chapel, and wrote many controversial books and pamphlets. In after times he wrote 'Letters to a Philosophical Institution;' 'An Answer to Gibbon;' 'Disqusitions on Matter and Spirit;' 'Corruptions of Christianity;' 'Early Opinions on Christ;' 'Familiar Letters to the Inhabitants of Birmingham;' 'Two Different Histories of the Christian Church;' 'On Education;' 'Comparison of Heathen and Christian Phi-

losophy;' 'Doctrine of Necessity;' 'On the Roman Catholic Claims;' 'On the French Revolution;' 'On the American War;' beside twenty volumes of tracts in favour of the Dissenters and their rights. His general works fill twenty-five volumes,* of which only five or six are on scientific subjects: his publications being in all one hundred and forty-one (in one year ten), of which only seventeen are on scientific matters. He is one of the most voluminous writers of any age or country, and probably he is of all voluminous writers the one who has the fewest readers. This arises from the circumstance that, though his political opinions are shared by many, the bulk of his works are theological and metaphysical, but especially theological; and his religious opinions were confined to an extremely small class of persons. Indeed it may be questioned if he was not in several respects the only person who held his peculiar faith upon all points.

It happened, fortunately, that when he went to reside at Leeds in care of the Mill-Hill chapel, his house immediately adjoined a common brewery, and this led him to make experiments upon the fixed air copiously produced during the process of fermentation. It must be observed, that long before this time the great step had been made by Black of ascertaining that there are other permanently elastic fluids than our atmosphere, and which have properties wholly different from it. Cavendish, too, had very recently subjected both fixed and inflammable airs (carbonic acid and hydrogen gases) to accurate experiments, showing their relative specific gravities, and proving that they were of the same nature from what bodies soever they were obtained. The probability was, that other gaseous fluids existed in nature as well as those two and common air. The experimenter had, therefore, thenceforth, his attention directed to meeting with

* Edited by the affectionate care of an able and worthy man, Mr. Towell Rutt.

these: and an examination of all the products of mixture and of heat, by precipitation or evaporation, was now the natural course of experimental inquiry. At first, Priestley only tried in what way fixed air could be most easily combined with water, he published in 1772 a pamphlet upon the means of effecting this union, and the condensing process which he employed is used to this day. He soon after gave to the Royal Society his observations on different kinds of air, which ascertained the important fact, that atmospheric air, after having been corrupted by the respiration of animals or by the burning of inflammable bodies, is restored to salubrity by the vegetation of plants; and that if the air is exposed to a mixture of sulphur and iron filings, as in one of Hales's experiments, its bulk is diminished between a fourth and a fifth, and the residue is both lighter than common air and unfit to support life. This residue he called 'Phlogistic air;' afterwards it was called 'Azotic' or 'Nitrogen gas;' and Dr. Rutherford, of Edinburgh, as well as Priestley, though unknown to each other, discovered it about the same time. For these experiments the Copley Medal was, in 1773, justly awarded to him by the Royal Society.

The following year was destined to be the period of a discovery most important for science, and truly glorious for its author. Having exposed red-lead, or minium, in a close vessel to the sun's rays concentrated by a burning-glass, he found that an aëriform body, permanently elastic, was evolved, and that this air had the peculiar property of increasing exceedingly the intensity of flame. This gas he called 'dephlogisticated air,' upon the principle that the matter of heat and light, the phlogiston of Stahl, being abstracted from it by the return of the calx to its metallic state, which phlogiston was supposed by that theory to effect, the air had great avidity for phlogiston, and seized it from the inflammable bodies it came in contact with. This

most important discovery, which he thus connected with an erroneous theory, was made on the 1st of August, 1774. He afterwards discovered that its absorption by the lungs in the process of respiration gives its red colour to arterial blood, as it was proved to act through the substance of thin bladder; and he found that when plants grow in close vessels, and restore the purity of the air in which a candle has burnt or an animal breathed, they do so by evolving this pure air. The new nomenclature gave it the name of 'oxygen gas,' from the belief then generally entertained that it was the acidifying principle. Later experiments have proved that there is at least one great exception to this in chlorine, formerly called 'oxygenated muriatic acid;' but now found to be wholly without oxygen, and yet to have all the properties of an acid. But, indeed, water itself, and the atmospheric air, having neither of them the nature of acids, are both contrary to the theory; and the fixed alkalis are found to owe their alkaline state and lose their metallic, like other oxides, by uniting with oxygen.

Priestley is the undoubted discoverer of oxygen. He was the first who communicated a knowledge of it to Lavoisier, at Paris, soon after he had made the discovery; nor can anything be more disingenuous than that celebrated person's afterwards affirming that he, Priestley, and Scheele, had all discovered it "about the same time." He never discovered it until Priestley discovered it to him. Bergmann's suppressing in his book all knowledge of the experiments of Black and Cavendish, the former published twenty and the latter eight years before, was bad enough, but not so bad as Lavoisier's positive assertion contrary to what must have been his positive knowledge.

This great discovery was far from being the last of its justly celebrated author. He discovered the gases of muriatic, of sulphuric, and of fluoric acids, ammonial gas, and nitrous oxide gas. He also discovered the

combination which nitrous gas forms suddenly with oxygen; diminishing the volume of both in proportion to that combination; and he thus invented the method of eudiometry, or the ascertainment of the relative purity of different kinds of atmospheric air.

It must not be forgotten, in considering the great merits of Priestley as an experimentalist, that he had almost to create the apparatus by which his processes were to be performed. He, for the most part, had to construct his instruments with his own hands, or if he employed others, he had to make unskilful workmen form them under his own immediate direction. His apparatus, however, and his contrivances for collecting, keeping, transferring gaseous bodies, and for exposing substances to their action, were simple and effectual, and they continue to be still used by chemical philosophers without any material improvement. It was fortunate in this respect that he began his pneumatic inquiries with seeking for the means of impregnating water with carbonic acid; this inquiry naturally turned his attention to the contrivance of apparatus and generally of manipulations, serviceable in the examination of bodies whose invisible form and elastic state renders inapplicable to them the machinery of the old laboratory, calculated only for solids and liquids.

The pertinacity with which Priestley clung to the phlogistic theory is marvellous. It might have been expected, that the fact of a combustion leaving the residue, whether of two gases, or of a gas and an inflammable body, exactly equal in weight to the sum of the weights of the bodies burnt and which had disappeared in the process, would have been accepted as a proof that these two bodies had entered into an union, giving out the latent heat which had previously held the gaseous body or bodies in a state of aëriform fluidity. It might, in like manner, have been expected, that when a metal, by absorbing oxygen gas,

becomes calcined, and gains in weight precisely the weight of the gas which has disappeared, the calcination should be ascribed to the gas, and that the reproduction of the gas by heat, or its abstraction by elective affinity for some other body, should be allowed to have restored the metallic state by simply destroying the union of the gas and the metal which had changed that metal. But nothing could overcome Priestley's repugnance to give up phlogiston: he adhered to it while he lived; he never would believe that water was formed of the two gaseous bodies whose combustion and disappearance leaves a weight of liquid equal to their joint weights; he always imagined that water was held in suspense by these gases and precipitated on their disappearing. He never would believe that metals owe their malleability and lustre to any cause other than phlogiston, or lose their properties except by oxygen taking the phlogiston from them. He never would believe that combustion is anything but the phlogiston leaving the inflammable body and joining the oxygen; or that when an acid is formed by the burning, that acid contains the oxygen and the combustible base. That his obstinate unbelief was perfectly disinterested no one can doubt. The discoverer of oxygen, and of the true cause of respiration, had, of all men, the strongest interest in assenting to a theory which was wholly founded upon his own discovery, and which made him the immediate, as Black was the more remote, author of modern chemical science—made him the philosopher who had raised the superstructure upon the foundation which his predecessor had laid.

The merit of Dr. Priestley, as a cultivator of science, was the activity with which he made experiments—the watchful attention with which he observed every phenomenon, following the minutest circumstances of each process—the versatility with which he prosecuted each new idea that arose from his trials—his

diligence in recording all the particulars, as if well aware how much depends in every branch of inductive philosophy upon allowing no fact to escape, when we are confessedly in search of light, and can never tell how any given fact may bear on the unknown conclusion to which our analytical process is leading us. As a reasoner his powers were far less considerable. He possessed not the sound judgment, the large circumspection, which enables men to weigh the relative value of either reasons or facts. He was cautious enough, and drew little from his imagination in feigning hypotheses, if it be not the reasons which he invented from time to time for the purpose of sustaining the desperate fortunes of the phlogistic theory, and making the facts bend to it as they successively arose, with a force capable of shivering it in pieces. But he was also deficient in the happy sagacity which pierces through apparent dissimilarity, and ranges things seemingly unlike under the same class—he had not that chastened imagination which can see beyond the fact present to the senses—in a word, he was much greater as a collector of new facts than a reasoner upon them—and his inductive capacity was inferior to his power of experimenting and of contriving the means of observation. Perhaps his want of general scientific acquirements, and his confined knowledge of chemistry, itself contributed to the activity and the boldness with which he performed novel experiments, while the same defect impaired his capacity as an inductive philosopher. It is extremely probable that the strict attention to principle, the methodical systematic spirit which presided over the inquiries of Black and of Cavendish—the scientific views which directed the contrivance of all their processes, never leading them to make any trial without some definite object in view, prevented them from performing many experiments, —from stooping, as it were, to try things which Priestley did not disdain to try from his more empiri-

cal turn of mind—what Mr. Watt, in a letter, calls "his random haphazarding."

In 1779, when Captain Cook was preparing to sail upon his second voyage, Mr. Banks, who took a great interest in it from having been engaged in the first, invited Dr. Priestley to accompany the Captain as astronomer to the expedition. Advantageous terms were proposed, including a provision for his family. He entertained the proposal, and then agreed to it; but objections were taken by the clerical members of the Board of Longitude, not to his ignorance of astronomy and of natural history, but to his Socinian principles in religion, which one might have supposed could exercise but a limited influence upon his observations of the stars and of plants. I know not if the same scientific authorities objected, on like grounds, in the council of the Royal Society, to receiving papers upon his chemical discoveries. It is certain that a like influence prevented Professor Playfair from afterwards proceeding to India, where he had designed to prosecute his inquiries into the science of the Hindoos. Such passages stamp the history of a great nation with indelible infamy in the eyes of the whole world.

In 1773, when his fame had been established by his first discoveries, and the Royal Society had crowned his paper with their medal, Priestley accepted an invitation from Lord Shelburne, afterwards first Marquis of Lansdowne, to fill the place of librarian and philosophic companion, with a salary of £250, reducible to £150 for life should he quit the employment. An additional allowance of £40 a-year was given by this truly munificent patron for the expense of apparatus and experiments; homes were provided for his family in the neighbourhood both of Lord Shelburne's town and country residence; nor can anything be easily conceived more truly gratifying to a man of right feelings, and of a noble ambition, than the reflection must have been, that the discovery of oxygen was

made under his roof, and with the funds which his disinterested liberality had provided for his learned guest. With whatever difference of sentiments statesmen may at any time view Lansdowne House, the lovers of science in after ages will gaze with veneration on that magnificent pile, careless of its architectural beauties, but grateful for the light which its illustrious founder caused to beam from thence over the whole range of natural knowledge; and after the structure shall have yielded to the fate of all human works, the ground on which it once stood, consecrated to far other recollections than those of conquest or of power, will be visited by the pilgrim of philosophy with a deeper fervour than any that fills the bosom near the forum or the capitol of ancient Rome.

In 1780 Priestley settled at Birmingham, where he was chosen minister of the principal dissenting congregation. He had left Lansdowne House without any difference to interrupt the friendship of its inmates; and some years afterwards an offer to return, made on the death of Lord Lansdowne's friends, Dunning and Lee, was declined.* A subscription among his friends furnished the means of prosecuting his experimental researches; and he declined an offer to obtain for him a pension from the government. A shade is cast upon this passage of his history by the circumstance of the pecuniary aid which he thus received being only in a small part rendered necessary for his experimental pursuits. Mr. Parker, the eminent optician, furnished him for nothing all the instruments made by him, as did Mr. Wedgwood all his earthenware utensils. Yet we find in his correspondence a painful thankfulness expressed, in any thing rather than the language of a

* This offer, and Lord Lansdowne's frank declaration that he never had any fault to find with his guest, entitles us to state that no quarrel, nor anything like it, had occurred. Nevertheless, Priestley's offer to visit his Lordship when he occasionally came to London was politely declined. Political reasons apparently caused this refusal.

philosopher, to Mrs. Rayner and Mr. Lee, for "seasonable benefactions." The "apology" which he evidently feels required for this kind of dependence is not at all confined to the "expense of his philosophical and theological studies;" he refers also to the education of his children, and to the expenses of housekeeping occasioned by his reputation.* It is not invidious to observe that, be a man's celebrity ever so great, he is not bound to incur any expenses in keeping hospitality, if these, "exceeding twice his own income" (and that, with the pension of Lord Lansdowne, not an inconsiderable one), can only be met by the large "benefactions" of his friends. He names fifteen who gave him by subscription a yearly allowance, all the while he chose to decline an offer made to procure a pension from the government, "wishing to preserve himself independent of every thing connected with the court." We must on this be content to remark, that different men entertain different notions of independence.

Settled at Birmingham, he continued, however, his controversial writings, and engaged eagerly in conflict with Gibbon upon his celebrated chapters respecting the Early History of Christianity, and with Bishop Horsley upon the Socinian doctrines. In the latter controversy the Episcopal and the Sectarian temperature, both high, were not very unequal; but in the former the minister of the Gospel had all the heat to himself—at least in the layman it was latent, if it existed at all. He was desirous of drawing his adversary into a controversy, and, failing in this, lost his temper, and had the vulgar recourse to calling names and imputing motives. Mr. Gibbon may have shown some superciliousness in his treatment of this angry polemic; but he certainly had a good right to marvel at the intolerance of one whose heterodoxy was so

* Memoirs, vol. i., part i., p. 217.

universal as to "condemn by circumscribing the inspiration of the Evangelists, and to condemn the religion of every Christian nation as a fable less innocent, not less absurd, than Mahomet's journey to the third heaven." How fortunate it was that Priestley lived in an age when the use of actual fire is withheld from theological disputants, as a mode of argumentation, must appear from the wonder he expresses at David Hume's monument having been so long suffered to offend the pious eyes of the Edinburgh people—an expression which might seem to convey a hint that he would have taken care to avoid, after he had himself felt the weight of the popular hand when called in to settle theological disputes.

Having taken, as was his wont, an active but not a very temperate part in the controversy to which the French Revolution gave rise, and having published a 'Reply' to Mr. Burke's famous pamphlet, he was early in 1791 made a citizen of the French Republic. An ironical and somewhat bitter pamphlet against the high church party still further excited the feelings of the people against him; and a dinner being given on the 14th of July to celebrate the anniversary of the attack upon the Bastille, the mob attacked the tavern where the party were assembled. Dr. Priestley was not present, but his chapel and house were immediately after assailed. His library, manuscripts, and apparatus were destroyed; his person and his family escaped. The compensation which he obtained, by an action against the hundred, fell short, according to his own account, by £2000 of his loss. As, however, an ample subscription was made for him, and as his brother-in-law generously gave him £10,000 with an annuity of £200 for life, he could not be other than a large gainer by the execrable violence of which he had been the victim; and as he never allowed any of his writings to remain unpublished for even the shortest time after they were finished, it is not likely that any

loss of an irreparable kind was incurred by the burning of his papers. He found, however, that he could no longer reside with comfort in the scene of such outrageous proceedings, and among a community which had so shamefully countenanced them. He removed to London, and succeeded his friend, Dr. Price, as Principal of the Hackney Academy. Late in the month of September, 1792, he was elected by the department of the Orne a member of the National Convention, about to assemble after the subversion of the French monarchy. This singular honour bestowed on him, as well for his philosophical fame as for his political services and the persecutions to which they had exposed him, he respectfully declined, giving as his reason that he was not familiar with the French language, and had not devoted his time sufficiently to legislative duties. But this moderation disarmed not his enemies—he was pursued by the intolerant spirit of the times. He found himself shunned by his former associates in science. Even the Royal Society did not afford an exception to this persecuting loyalty, or a shelter from its effects; and in the spring of 1794 he withdrew to America. Here he again suffered considerable disappointment. His religion was too much for those who had ceased to care for sacred things, and far too scanty for those who still were Christians, while his republican opinions were exceedingly distasteful because they were tinged with a decided admiration of France. He continued, however, to inhabit the country, and to prosecute his studies, chiefly theological. He received contributions regularly from his benefactors in England, Mrs. Rayner and the Duke of Grafton; but these, though acknowledged by him in the same unpleasant style as eleemosynary ("very acceptable benevolences"), were for the most part on a different footing from the English charities; they appear generally to have been required for the propagation of their

Unitarian opinions, to which the parties were all so zealously attached.

He settled at Northumberland, in an uncleared district, where he purchased three hundred acres of land; and his youngest son, Henry, then a very fine young man of eighteen, devoted himself to the clearing and cultivating this woodland spot, working with his labourers and sharing their toils. The father himself partook of this labour for two or three hours daily. On Sundays he frequently preached, and when he visited Philadelphia he always did so. He devoted the rest of his time to his works, particularly his 'Church History;' and he wrote answers to Paine and Volney. He was much obstructed in his philosophical pursuits by the want of proper accommodation for his apparatus, and he only wrote three tracts on chemical subjects during the ten years of his residence in America; two of which were merely arguments on phlogiston, and the third alone recorded any experiments; it was written eight years before his death.

At the end of 1795 he suffered a heavy affliction in the death of his son Henry, after a few days' illness; and in ten months more he also lost his wife. These blows, though he felt their weight, did not at all crush him; his resignation was exemplary; and his steady, enthusiastic faith in Revelation gave him a certain hope of meeting, before many years should elapse, with those whom he had lost. Indeed, his letters clearly show that he regarded the sundering of these ties far less attentively than their restoration. A few days after his son's death he writes to his most intimate friend and constant correspondent, Theophilus Lindsay, recounting the particulars of his loss, and he adds that he is composing three discourses on Revelation against modern unbelievers. The letter next year announcing his wife's death, begins with saying to the same friend how much he stands in need of his sympathy, and goes on to add, "This day I bury my wife; she died

on Saturday after an illness of a fortnight." He adds some remarks on his literary occupations, and concludes with mentioning a plan he has of travelling to distract his mind.* No one who reads his letters and his memoirs, written by himself, can doubt that this stoical firmness is not the result of a callous disposition, but the signal triumph of a heartfelt belief in the promises of Religion over the weakness of our nature.

It is, indeed, quite manifest that Religion was as much an active principle in him as in any one who ever lived. Not only is it always uppermost in his thoughts, but he even regards temporal concerns of a public nature always in connexion with the Divine superintendence, and even with the prophecies of Scripture. His letters are full of references to those prophecies as bearing on passing events, and he plainly says that since his removal to America he should care little for European events but for their connexion with the Old Testament. He also looked for an actual and material second coming of Christ upon earth.

It is not true to affirm that he was little of a politician, though in declining the seat in the National Convention he says † his studies had been little directed towards legislation compared with theology and philosophy; and denies in a letter to William Smith that he ever taught or even mentioned politics to his pupils, as he had been charged with doing, among the innumerable falsehoods of which he was the subject. Nor is the circumstance of his not attending political meetings at all decisive of his being little of a political agitator, because his incurable stutter prevented him from taking a part in such proceedings. But he wrote in 1774, at Franklin's request, an address to the people on the American disputes,

* Mem. vol. i., part ii., p. 328, 354.
† Mem. vol. i., part ii., p. 190—198.

previous to the general election. He answered Mr. Burke's 'Reflections on the French Revolution.' He mixed in the question of the Catholic claims; and he published in all no less than eleven political works, almost every one upon the topics of the day. It is equally true, however, that theological controversy occupied him far more constantly and engaged his mind far more deeply than political matters; that he was regularly a theologian and incidentally a partisan.

The cast of his political opinions had originally little more tendency to democracy than those of Whigs who have read and discussed more than they have reflected and seen. He used, indeed, to say that in politics he was a Trinitarian, though a Unitarian in religion. It must, however, be confessed that he went very much further in the same direction after the French Revolution had set fire to the four quarters of the political world, and his admiration of republican principles might be measured by his zeal for the innovators of France, with the success of whose arms he deemed the safety of freedom to be bound up. When we read his answer to the offer of a seat in 1792, and reflect that it was penned about three weeks after the horrible massacres of September, the worst of the atrocities which disfigured the Revolution, it moves our wonder to find a Christian minister accompanying his acknowledgment of the honour proposed, that of being enrolled among the authors of the tragedy so recently enacted, with no protest against the bloody course then pursuing, no exception to the unqualified admiration expressed of the youthful republic.

In America we find his leanings are all against the Federal party, and his censures of the great Chief of the Union little concealed. He felt for the democratic party, the French alliance, the enemies of English partialities, and he regarded Washington as ungrateful because he would not, from a recollection of the services of France twenty years before to American in-

dependence, consent to make America dependent upon France. The indifferent reception which he met with in society was probably owing to this party violence full as much as to the dislike of his Unitarian opinions. But it must be added, that his temper was so mild, and his manners so gentle, as to disarm his most prejudiced adversaries whensoever they came into his society. Many instances of this are given in his correspondence, of which one may be cited. He happened to visit a friend whose wife received him in her husband's absence, but feared to name him before a Calvinistic divine present. By accident his name was mentioned, and the lady then introduced him. But he of the Genevan school drew back, saying, "Dr. Joseph Priestley?" and then added in the American tongue, "I cannot be cordial." Whereupon the Doctor, with his usual placid demeanour, said that he and the lady might be allowed to converse until their host should return. By degrees the conversation became general; the *repudiator* was won over by curiosity first, then by gratification; he remained till a late hour hanging upon Priestley's lips; he took his departure at length, and told the host as he quitted the house, that never had he passed so delightful an evening, though he admitted that he had begun it "by behaving like a fool and a brute." One such anecdote (and there are many current) is of more force to describe its subject than a hundred laboured panegyrics.

After the loss of his wife and his younger and favourite son, he continued with unabated zeal to pursue his theological studies, and published several works, both controversial and historical, beside leaving some which have been given to the world since his decease. He endeavoured, too, as far as he could, to propagate the tenets of Unitarianism, and to collect and extend a congregation at Philadelphia attached to that doctrine. At one time, in the summer of 1797, entertaining hopes of peace in Europe, he had resolved to

visit France, where he might communicate personally with his English friends; and he even thought of making a purchase in that country on which he might reside during a part of each year. So nearly did he contemplate this removal, that we find him desiring the answers to letters he was writing might be sent to the care of Messrs. Perregaux at Paris. The revolution of Fructidor, however (4th September, 1797), put an end to all prospects of peace, and the war soon raged in every quarter with redoubled fury. He seems now to have derived his chief comfort from tracing the fancied resemblance between the events passing before him and the prophecies in Scripture; though occasionally he felt much puzzled, and the book of Daniel, especially, appears to have given him trouble and perplexity. When the peace came at last, his health was too much broken to permit any plans to be executed such as he had four years before contemplated.

In 1802 he became a confirmed invalid, suffering from internal, and apparently organic, derangement. His illness was long and lingering, and he suffered great pain with perfect patience for two years. The prospect of death which he had before him did not relax his application to literary labour, his faculties remaining entire to the last. Neither did that awful certainty, ever present to his mind, affect him with sorrow or dismay. The same unshaken belief in a future state, the same confident hope of immortal life which had supported him under his affliction for the death of others, cheered him while contemplating the approach of his own. In this happy frame of mind he gently expired on the 6th of February, 1804, in the seventy-second year of his age.

His character is a matter of no doubt, and it is of a high order. That he was a most able, most industrious, most successful student of nature, is clear; and that his name will for ever be held in grateful remembrance by all who cultivate physical science, and

placed among those of its most eminent masters, is unquestionable. That he was a perfectly conscientious man in all the opinions which he embraced, and sincere in all he published respecting other subjects, appears equally beyond dispute. He was, also, upright and honourable in all his dealings, and justly beloved by his family and friends as a man spotless in all the relations of life. That he was governed in his public conduct by a temper too hot and irritable to be consistent either with his own dignity, or with an amiable deportment, may be freely admitted; and his want of self-command, and want of judgment in the practical affairs of life, was manifest above all in his controversial history; for he can be charged with no want of prudence in the management of his private concerns. His violence and irritability, too, seems equally to have been confined to his public life, for in private all have allowed him the praise of a mild and attractive demeanour; and we have just seen its great power in disarming the prejudices of his adversaries.

CAVENDISH.

A GREATER contrast between two men of science, both eminent benefactors to the same branch of knowledge, can hardly be imagined than Cavendish offers to Priestley. He was thoroughly educated in all branches of the Mathematics and Natural Philosophy; he studied each systematically; he lived retired from the world among his books and his instruments, never meddling with the affairs of active life; he passed his whole time in storing his mind with the knowlege imparted by former inquirers and in extending its bounds. Cultivating science for its own sake, he was slow to appear before the world as an author; had reached the middle age of life before he gave any work to the press; and though he reached the term of fourscore, never published a hundred pages. His methods of investigation were nearly as opposite as this diversity might lead us to expect; and in all the accidental circumstances of rank and wealth the same contrast is to be remarked. He was a duke's grandson; he possessed a princely fortune; his whole expenditure was on philosophical pursuits; his whole existence was in his laboratory or his library. If such a life presents little variety and few incidents to the vulgar observer, it is a matter of most interesting contemplation to all who set its just value upon the cultivation of science, who reckon its successful pursuit as the greatest privilege, the brightest glory of our nature.

Henry Cavendish was born at Nice, whither his mother's health had carried her, the 10th of October, 1731. He was the son of Lord Charles Cavendish, the

late Duke of Devonshire's great uncle, by the daughter of Henry Grey, Duke of Kent. His family, aware of the talents which he early showed, were anxious that he should take the part in public life which men of his rank are wont to do, and were much displeased with his steady refusal to quit the studies which he loved. An uncle, disapproving of the course pursued towards him made him his heir; and so ample a fortune came into his possession that he left at his death a million and a quarter of money.* The Mathematics, and the various branches of Natural Philosophy, were the chief subjects of his study, and of all these sciences he was a consummate master.

The discoveries of Black on carbonic acid and latent heat, appear to have drawn his attention to the cultivation of pneumatic chemistry; and in 1766 he communicated to the Royal Society his experiments for ascertaining the properties of carbonic acid and hydrogen gas.† He carried his mathematical habits into the laboratory; and not satisfied with showing the other qualities which make it clear that these two aëriform substances are each *sui generis*, and the same from whatever substances, by whatever processes, they are obtained; nor satisfied with the mere fact that one of them is heavier, and the other much lighter, than atmospheric air,—he inquired into the precise numerical relation of their specific gravities with one another and with common air, and first showed an example of weighing permanently elastic fluids: unless, indeed, Torricelli may be said before him to have shown the relative weight of a column of air and a column of mercury: or the common pump to have long ago

* M. Biot's article in the Biog. Univ. makes him the son of the Duke of Devonshire, and states his yearly income at £300,000 sterling, and yet gives the property he left at only £1,200,000—so that he must have spent £300,000 a-year, and also dissipated five millions. Such errors seem incredible.

† Three papers containing experiments on factitious air. Phil. Trans. 1766, p. 141.

compared in this respect air with water. It is, however, sufficiently clear, that neither of these experiments gave the relative measure of one air with another: nor, indeed, could they be said to compare common air with either mercury or water, although they certainly showed the relative specific gravities of the two bodies, taking air for the middle term or common measure of their weights.

The common accounts in chemical and in biographical works are materially incorrect respecting the manner in which Mr. Cavendish was led to make his great experiment upon the composition of water in 1781 and the following years. It is said, that while making his experiments on air in 1765 and 1766, he had observed for the first time, that moisture is produced by the combustion of inflammable air, and that this led him, sixteen or seventeen years later, "to complete the synthetical formula of water, and to find that the moisture that he had before observed was simple water."* Nothing can be more erroneous than this whole statement. In Mr. Cavendish's paper, of 1766, upon fixed and inflammable airs, there is not one word said of the moisture formed by the combustion; and respecting inflammable air, the experiments are confined entirely to its burning or exploding, to its specific gravity, and to its production. The paper of 1784 is, in fact, entitled 'Experiments upon Air,' and it commences with stating, not that those experiments were undertaken with any view to the water formed by burning inflammable air, but that they were made "with a view to find out the cause of the diminution which common

* Penny Cyclopædia, vol. vi., p. 392. This and other similar accounts are plainly given by some persons who never read Mr. Cavendish's writings. But a still greater error occurs in them; they represent him as having first shown that fixed and inflammable airs are separate bodies from common air; whereas Dr. Black, in his Lectures from 1755 downwards, showed this distinctly by his experiments, proving clearly that these gases have nothing in common with the atmospheric air (vol. ii., p. 87, 88).

air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or consumed;" and the author adds, that besides, "determining this fact, they also threw light on the constitution and means of production of dephlogisticated air." Instead of referring to any former observation of his own either in 1766, or subsequently, on the moisture left by burning inflammable air, he expressly refers to Mr. Warltire's observation of this moisture, as related by Dr. Priestley: and both Mr. Warltire's observation and Dr. Priestley's publication were made in 1781. Upon this observation Mr. Cavendish proceeded to further experiments, with the view of ascertaining "what becomes of the air lost by phlogistication." For this purpose, he introduced a portion of hydrogen gas into a globe or balloon of glass, sufficiently strong to resist the expansive force of the combustion which had often been observed in mines, and also in experiments upon a smaller scale, to produce an explosion. He adapted to the globe two wires of metal, fixing them in air-tight sockets, and bringing their points within a short distance of each other in the inside of the globe; so that, by an electrical machine, he could send the spark or the shock from the one point to the other, through the gases mixed together in the globe. He found that the whole of the hydrogen gas disappeared by the combustion thus occasioned, and a considerable portion also of the common air. Water was, as usual, found in small quantity, and an acid was also formed. He then weighed accurately the air of both kinds which he exposed to the stream of electricity, and he afterwards weighed the liquid formed by the combustion; he found that the two weights corresponded with great accuracy. It was difficult to resist the inference that the union of the two airs had taken place; and it might further have been inferred that the latent heat which held them in an elastic state had been given

out, forming the flame which was produced; and that water was formed by the union of the two airs, having, of course, less latent heat than was required to keep them in a gaseous state; but Mr. Cavendish did not approve of this manner of stating the conclusion which Mr. Watt had adopted, because of doubts which he had respecting the nature of heat.* The residue of the combustion, however, was two-fold: there was an aëriform body left in the glass vessel, as well as liquid in the bottom. This was much smaller in volume than the air which had filled the globe before the combustion, because the hydrogen gas and part of the common air had disappeared. This aëriform residue was also of a different nature from common air; it was found to be the phlogistic air of Priestley; the azotic air of Rutherford: and the air consumed in burning the hydrogen gas must, therefore, be the vital air or oxygen gas of the atmosphere. By another experiment he more fully ascertained this: for, burning oxygen gas with hydrogen gas, nearly the whole aëriform contents of the globe disappeared, and water, equal in weight to the two gases taken together, remained as the produce of the combustion; but still an acid was formed, unless in some cases, when very pure oxygen gas was used.

Thus was effected the important discovery of the composition of water, which Watt had inferred some time before from a careful examination of the similar facts collected by former experimentalists; one of whom, Warltire, had even burned the gases in a close vessel, and by means of electricity. The conclusion arrived at by Mr. Cavendish from his capital experiment was, in his own words, that "dephlogisticated air is in reality nothing but deplogisticated water, or water deprived of its phlogiston, or in other words, that water consists of dephlogisticated air united to

* Page 140.

phlogiston, and that inflammable air is either pure phlogiston, or else water united to phlogiston;" and he then gives his reasons in favour of the second inference, namely, that inflammable air is water united to phlogiston; but he repeatedly dwells on the preference due to this inference over the conclusion that inflammable air is pure phlogiston.* This statement of the theory is somewhat less distinct than Mr. Watt's, who considered water to be dephlogisticated air united to inflammable air or pure phlogiston, and both deprived of their latent heat. But he, as well as Mr. Cavendish, expresses himself with some hesitation, and even, like him, in some passages entertains the idea of water as united in a small proportion with inflammable air. The theory, though nearly completed by those great chemists, was perhaps first stated with perfect certainty and distinctness by Lavoisier.†

In the combustion of hydrogen gas with common air, and even with impure oxygen gas, Cavendish had observed that the water was slightly tinged with acid, though not always when pure oxygen gas was used for the operation. He therefore devised an experiment which should ascertain the nature of this acid, and in what manner it was formed. He passed the electric spark through common air without any hydrogen gas being present; the air was in a receiver over mercury, and the operation was of long continuance, on account of the slowness with which the combination is formed of the two gases whereof the atmosphere is composed. He had not supposed that the hydrogen had any share in forming the acid: his theory being that water, and not acid, is the produce of that gas's combustion. He naturally suspected the acid to be the produce of some union between the azote and the oxygen of the atmosphere. He left the process in the hands of a committee of his scientific

* Philosophical Transactions, 1784, p. 137, 140.

† See Appendix to the Life of Watt.

friends, fellow-members of the Royal Society; and after some weeks of constantly passing the electric fluid through a limited portion of air, a small quantity of liquid was formed, which readily combined with a solution of potash in water sent up through the mercury. This union was found to be common nitre, having all the qualities of that well-known substance. It detonated with charcoal; it sparkled when paper impregnated with it was burnt; it gave out nitrous fumes when sulphuric acid was poured on it. There could, therefore, no doubt whatever now exist that nitrous acid is composed of the two airs deprived of latent heat, which form our atmosphere; that it is a true oxide of azote.

The undivided merit of this important discovery has never been denied to Mr. Cavendish. Even Lavoisier could not intrude; but his avidity to claim a share in all discoveries had been exerted respecting the composition of water, which he asserts in his 'Elements of Chemistry' to have been discovered by himself and Mr. Cavendish about the same time. I have shown clearly in the Appendix to the Life of Mr. Watt, that the discovery had been previously communicated to the French philosopher; but it is worth while to consider the experiment upon which he grounded his claim; and that experiment, when examined, is found wholly insufficient to prove the position, even if it had been contrived and performed before the communication of Watt's and Cavendish's discovery. Of that discovery it was plainly a corollary—by that discovery it was manifestly suggested.

The former experiments, both those of Cavendish and those on which Watt reasoned, were all synthetical and decisive—that of Lavoisier was analytical and radically defective. It proved nothing conclusively: it was well enough after the *experimentum crucis* had demonstrated the proposition; to that proposition it was a corollary—it was nothing like a critical experi-

ment. He placed water in a retort exposed to heat; the vapour of the retort, when the water boiled, was passed through a tube (a gun barrel with the breech-pin knocked out was generally used); the tube, if made of earthenware, had iron filings placed in its course; it was placed in a fire; its further extremity was connected with a receiver, in which cold water or mercury rose to fill it entirely. As the water slowly boiled there came through the tube, and into the glass receiver, a current of gas, which, upon examination, was found to be hydrogen gas, while the iron filings were converted into calx or oxide. The weight of the gas produced, added to the weight acquired by the gun barrel or by the filings during the process, was found to be nearly equal to the weight lost by the water in the retort. Hence the inference was, that the lost portion of water had been decomposed into its two elements, the oxygen gas forming the calx of the iron and the hydrogen gas being received in the glass vessel. But the adversaries of the new doctrine had an answer to this inference far more formidable than any that they could urge against the conclusion drawn from the synthetical experiment. The analytical experiment was liable to all the uncertainty of the process called the destructive distillation. The substances found might have been the product, and not merely the educt of the process. It is known that if coal or oleaginous bodies be distilled in close vessels there are obtained gases and water and acids which never existed in the matters subjected to the action of the fire. The component parts of these matters enter into new combinations with one another under the action of heat, just as a tallow candle or an oil lamp gives lamp-black and water in burning, though no water, but only hydrogen, nor of course any lamp-black, exists in the tallow and the oil. So, in Lavoisier's experiment, the water might contain only oxygen and hydrogen, and the action of the hot iron might

have separated them from each other. But it was also quite possible that the iron gave out hydrogen, and that the hot water was partly kept in solution by this gas, partly combined with the iron, for on that supposition the combined weight of the calcined iron and the hydrogen gas would be exactly equal to the united weight of the water evaporated, and of the iron before calcination. The previous discovery of Watt and Cavendish is liable to no such ambiguity; and it has the merit of also removing all ambiguity from the experiment of Lavoisier, which it manifestly suggested.

These great discoveries placed Cavendish in the highest rank of philosophers. No one doubted of nitrous acid; that he was the undisputed discoverer of the composition of water, before Mr. Watt's claim, is equally certain; nor, even now, is it necessary for the defenders of Watt's priority to deny that Cavendish made the great step without any previous knowlege of Watt's reasoning, while all admit that his *experimentum crucis* was of the greatest value in completing the foundation on which Watt's happy inference had been built. Lavoisier's attempt to intrude himself was wholly unsuccessful; it had no effect whatever except to tarnish his reputation, already injured sufficiently by his similar attempt to share in the discovery of oxygen. All men held Cavendish as placed among the greatest discoverers of any age, and only lamented that he did not pursue his brilliant career with more activity, so as to augment still farther the debt of gratitude under which he had laid the scientific world.

The reader, especially the French reader, must not suppose that any prejudice respecting Lavoisier has dictated the remarks occasionally made in the course of this work upon his pretensions as a discoverer. It is scarcely possible to estimate too highly the services which he rendered to chemical science by his labours. The truly philosophic spirit which guided his researches had not been found to prevail much before his time in

the speculations of chemists. He had a most happy facility in reducing the knowledge of scattered and isolated facts to a system. His talent for generalization has not often been surpassed; and it led him, together with his admirable freedom from preconceived prejudice, and his resolute boldness of investigation in unfrequented paths, to make some of the most felicitous inductions, well deserving the title of discoveries, that have ever been made, although the materials of his inferences were obtained from the experiments and observations of his predecessors, and his own experiments, except on the nature of the diamond, led to no material extension of our chemical knowledge. Stript of the plumes in which he sought to array himself, repulsed from the avenues by which he would fain have intruded himself among those whose experiments led at once to great discoveries, he is now, on all hands, allowed to have never made us acquainted with a single new gas, or a new substance of any kind, or, except as to carbon, with a single new combination of the old. He did not, like Black, discover carbonic acid or latent heat—he did not, like Priestley, discover oxygen—he did not, like Scheele, discover chlorine—he did not, like Davy, discover the alkaline metals—or like Cavendish, by direct experiment, show how water and nitrous acid are constituted—or, like Berthollet, explain of what ammonia consists. But it is equally confessed that, by sound and happy reasoning on the experiments of others, he showed how the process of combustion and of calcination takes place, and to him and his individual researches we owe the important discovery that fixed air, however generated, whether by respiration or by combustion or by fermentation (its three great sources, as proved by Black), is the combination of oxygen and carbon. Nor is it any derogation from his claims to the title of a discoverer of physical truths that his generalization pushed too far made him regard oxygen as necessary to all combustion and all acidi-

fication, whereas it has been found that heat and light are abundantly evolved both by the combustion of metals and sulphur in close vessels—by the combustion of hydrogen and azotic gas—and by the combination of metals with chlorine; and also that chlorine, an acid of the strongest kind, contains no oxygen at all, while the alkalis themselves are oxides. The doctrine of latent heat was happily applied by him to the union of gases with bodies, and if he had only followed that doctrine more closely he would have avoided the error into which he fell, and have perceived that other gases as well as oxygen may support flame, and that all, on becoming liquid or solid, must part with heat. Against his error respecting the constitution of acids may justly be set the great merit of his conjecture, that the fixed alkalis are oxides of metals; for this has been since proved, and the conjecture is a sufficient evidence that he did not doggedly adhere to his theory of the acidifying principle.

It does not appear that Mr. Cavendish ever after 1785, when he discovered the nature of nitrous acid, prosecuted his chemical inquiries so as to make new discoveries; but beside making numberless useful chemical experiments, about ten years later he engaged in some important experiments upon the force of attraction. It occurred to him that he could measure that force, and thereby ascertain the density of the earth by accurately observing the action of bodies suddenly exhibited in the neighbourhood of a horizontal lever nicely balanced, loaded with equal leaden balls of a small size at its two ends, and protected from all aërial currents by being inclosed in a box. In that box a telescope and lamp were placed, that the motions of the lever might be carefully observed. On approaching the external leaden balls made use of, whose diameter was eight inches, to the small ones inclosed, and near the lever, it was found that a horizontal oscillation took place. This was measured;

always employs the Newtonian notation) for the subnormal, having taken x for some other quantity than the abscissa, and using three letters, as a, z, and x, to denote segments of the same line, when perhaps a is the whole line, and $a - x$ is equal to z. But that he had the most familiar and masterly knowledge of the calculus is plain throughout all his investigations, as it is that his trust in its powers induced him to throw himself willingly and habitually upon them. In this respect he stands not only at the head of chemical philosophers, but alone among them, with perhaps one or two exceptions in the French school.

In giving the history of his labours, and the character of his intellectual capacity, we have written the life of Cavendish. His personal history cannot be expected to have any striking interest; yet they who have been dwelling on his scientific eminence will not be displeased to know somewhat of his ordinary life. He was of a most reserved disposition, and peculiarly shy habits. This led to some singularity of manner, which was further increased by a hesitation or difficulty of speech, and a thin shrill voice. He entered diffidently into any conversation, and seemed to dislike being spoken to. He would often leave the place where he was addressed, and leave it abruptly, with a kind of cry or ejaculation, as if scared and disturbed. He lived in a house on Clapham Common, and his library, vast in extent, was at another place, because he made it accessible to all, and did not wish to be troubled by those who resorted to it. He allowed friends to take books from it, and he himself never took one without giving a receipt for it. On the death of his librarian he began the practice of himself attending one day in the week to give out and take in books. His large income was allowed to accumulate; and when his bankers, after finding that a very considerable balance was always left in their hands, mentioned the circumstance, suggesting that it might be invested to

some profit, he answered with much simplicity, that if the balance was an inconvenience to them he could go to another banker. Himself a man of no expense, his habits never varied, nor did his style of living at all suffer a change on succeeding to his uncle's large fortune. His purse was ever accessible to the claims of charity, as well as to proposals for the promotion of scientific pursuits. Having formed a high opinion of Dr. (afterwards Sir Charles) Blagden's capacity for science, he settled a considerable annuity on him, upon condition that he should give up his profession and devote himself to philosophy; with the former portion of which condition the Doctor complied, devoting himself to the hopeless pursuit of a larger income in the person of Lavoisier's widow, who preferred marrying Count Rumford.* Mr. Cavendish received no one at his residence; he ordered his dinner daily by a note which he left at a certain hour on the hall table, where the housekeeper was to take it, for he held no communication with his female domestics, from his morbid shyness. It followed, as a matter of course, that his servants thought him strange, and his neighbours deemed him out of his mind. He hardly ever went into society. The only exceptions I am aware of are an occasional christening at Devonshire or Burlington House, the meetings of the Royal Society, and Sir Joseph Banks' weekly conversaziones. At both the latter places I have met him, and recollect the shrill cry he uttered as he shuffled quickly from room to room, seeming to be annoyed if looked at, but sometimes approaching to hear what was passing among others. His face was intelligent and mild, though, from the nervous irritation which he seemed to feel, the expression could hardly be called calm. It is not likely that he ever should have been induced to sit for his picture; the result, therefore, of

* He left Sir Charles a legacy of £15,000; which was generally understood to have fallen much short of his ample expectations.

any such experiment is wanting. His dress was of the oldest fashion, a greyish green coat and waistcoat, with flaps, a small cocked hat, and his hair dressed like a wig (which possibly it was) with a thick clubbed tail. His walk was quick and uneasy; of course he never appeared in London unless lying back in the corner of his carriage. He probably uttered fewer words in the course of his life than any man who ever lived to fourscore years, not at all excepting the monks of La Trappe.

Mr. Cavendish died on the 10th of March, 1810, after a short illness, probably the first as well as the last under which he ever suffered. His habit of curious observation continued to the end. He was desirous of marking the progress of disease, and the gradual extinction of the vital powers. With this view, that he might not be disturbed he desired to be left alone. His servant returning sooner than he had wished was ordered again to leave the chamber of death, and when he came back a second time he found his master had expired.

DAVY.

SIR HUMPHRY DAVY being now removed beyond the reach of such feelings, as he ought always to have been above their influence, that may be said without offence of which he so disliked the mention: he had the honour of raising himself to the highest place among the chemical philosophers of the age; emerging by his merit alone from an obscure condition. His father was a carver in wood at Penzance, in Cornwall; a man of some ingenuity in his craft. He possessed a small landed property in the village of Varfell, near Penzance, and Davy was born there in 1778. He received the rudiments of his education at a school in Truro, but was very early apprenticed to an apothecary at Penzance, where, disliking the profession to which he had been destined, he occupied himself with chemical experiments, ingeniously contriving to make the utensils of the shop and the kitchen serve for apparatus; and it is remembered of him that he frequently alarmed the household by his explosions. The result of his dislike to the shop was a disagreement with his master, and he went to another in the same place; but here he continued in the same course. Pursuing a plan of study which he had laid down for himself, he became thoroughly acquainted with chemistry, and well versed in other branches of natural philosophy, beside making some proficiency in geometry; but he never cultivated the mathematical sciences, except that I recollect his telling me once, late in life, of his intention to resume the study of them, as he had begun to make progress in crystallo-

graphy. He does not appear to have given any early indications of superior genius, or even of unusual quickness; but he showed all along, in following the bent of his intellectual taste, the perseverance, the firm purpose, which is inseparable from a capacity of the higher order, and is an indispensable condition, as it is a sure pledge, of success in every pursuit.

It must be observed of the biographers both of Davy and Scheele, that they seem to have made too much of the difficulties interposed in the path of their early studies by the want of apparatus, to which want, and to their ingenious contrivances for finding substitutes, a good deal of their experimental skill has been ascribed. It should be recollected that an apothecary's shop is not by any means so destitute of helps, especially for the study of chemistry, as a workshop of almost any other description. Crucibles, phials, mortars, gallipots, scales and weights, liquid measures, acids, alkalis, and neutral salts, are all to be found there, even if a furnace and still be not a necessary appendage. It may be allowed that nothing like an air-pump might be there expected, unless cupping chanced to be performed by the druggist. Accordingly Davy was glad to obtain, in a case of surgical instruments from a practitioner on board a French vessel wrecked on the Cornish coast, to whom he had done some kind service, the means of making some approximation to an exhausting engine.

It happened, fortunately for him, that Gregory Watt, youngest son of the great engineer, and whom, having had the happiness of knowing him, I have already mentioned, came to reside in the house of Davy's mother at Penzance, where he was ordered to pass the winter for the benefit of his health. Being five years older than the young chemist, and eminently accomplished both in science and in letters, his conversation and advice was a great advantage, of which Davy gladly availed himself. Another accident threw

him in the way of Mr. Davies Giddy, a cultivator of natural as well as mathematical science, and he, finding that Davy had been devoting himself to chemistry, gave him the use of an excellent library, and introduced him to Dr. Beddoes, who was then engaged in forming an establishment called by him the Pneumatic Institution, for the medical use of gases, as well as for further investigating their properties. At the head of this he placed his new friend, who was thus at once enabled to pursue his scientific vocation as a profession, and did not long delay giving to the world a proof of his ingenuity, by the publication of a theory of 'Light and Heat,' fanciful no doubt, and ill-digested, containing much groundless and imaginary, and even absurd speculation, but disclosing great information and no inconsiderable cleverness. It was published in a periodical work edited by Dr. Beddoes, called 'Contributions to Medical and Physical Science;' and to the same work he soon after gave a paper upon the 'Nitrous Oxide,' on the respiration of which he had made some very curious experiments. The singular circumstances which he thus ascertained, gave him considerable reputation as an experimentalist, and he was soon after (1802) chosen first Assistant Lecturer in Chemistry, by the Royal Institution of London, and the year following, sole Chemical Professor. Nor must the boldness which he had shown in conducting his experiments be passed over. He had exposed himself to serious hazard in breathing some most deleterious gases, and both in his trials of gaseous mixtures, and in his galvanic processes, he had made many narrow escapes from the danger of violent explosions.

It is a singular fact that, although his attention had never been confined to his favourite science, for he had studied literature, and especially poetry to the extent of writing tolerable verses, yet he was of so uncouth an exterior and manners, notwithstanding an exceedingly handsome and expressive countenance,

that Count Rumford, a leading director of the Institution, on seeing him for the first time, expressed no little disappointment, even regretting the part he had taken in promoting the engagement. But these feelings were of short duration. Davy was soon sufficiently humanized, and even refined, to appear before a London and a fashionable audience of both sexes with great advantage, and his first course of lectures had unbounded and unparalleled success. This he owed, certainly, to the more superficial accomplishments of good and lively language, an agreeable delivery, and, above all, an ingenuous enthusiasm for his subject which informed and quickened his whole discourse. But the fame which he thus acquired would have been of limited extent and of short duration, had his reliance only been upon the fickle multitude whom such qualities can please. The first consequences of his success in the line of mere exhibition were unfavourable, and threatened to be fatal; for he was led away by the plaudits of fashion, and must needs join in its frothy, feeble current. For a while he is remarked to have shown the incongruous combination of science and fashion, which form a most imperfect union, and produce a compound of no valuable qualities, somewhat resembling the nitrous gas on which he experimented earlier in life, having an intoxicating effect on the party tasting it, and a ludicrous one on all beholders. They who have recorded this transformation, while they lament the substitution of anything for "the natural candour and warmth of feeling which had singularly won upon the acquaintance of his early life," add most justly that the weakness which they describe never "cooled his regard for his family and former friends." I can vouch for the change, which was merely superficial, being of very short duration; and it is pleasing to add that, even while it lasted, there was none of that most offensive of all the effects produced by such a transition state to be found in his conversation; he

never for a moment appeared to be ashamed of his great vocation, nor to shun the fullest discussion of the subject on which he was at home, in order to deal with topics to which he was of necessity a stranger. I am speaking, too, of his habits long before his great discoveries; there would have been little ground for praise, any more than for wonder, that the discoverer of the alkaline metals should be willing to have the conversation roll upon chemistry and galvanism; but the time to which I have been referring was when his fame rested chiefly upon the success of his lectures to mixed companies in Albemarle-Street, and to lovers of agriculture in Sackville-Street, where the Board had chosen him their Chemical Professor.

If his situation at the Royal Institution had exposed him to the risk which we have seen he escaped, it had put him in possession of invaluable helps to his pursuits. He had now an ample command of books; he had assistants under him; above all, he had an unlimited power of collecting and of making apparatus: his income was secure; and his time was at his own disposal. He failed not to avail himself diligently of these great advantages; and although he lived a good deal in society, where he was always a welcome guest, his principal relaxations during the rest of his life consisted in shooting, and especially in fishing, of which he was from his earliest years passionately fond. The intercourse he had held with Southey and with Coleridge had given him not only his taste for poetry, but an extraordinary love of rural walks, in the peaceful solitude of which I have heard him say, answering the ordinary and obvious objections of those who are not smitten with the love of the "Angle," the gratifications of that propensity very mainly consist.

In 1801 he made his first important discovery, that by which he ascertained the true nature of galvanic action. That this was connected with electric or chemical affinity had been generally suspected, though

denied by Volta, the author of the pile, and indeed of the science which, like the continent of America, has borne the name of another than the discoverer. This had seemed probable from the presence being indispensable of a liquid capable of decomposing one or other of the metals, both supposed to be equally necessary to the production of the electric stream. Davy's experiments, which were numerous and admirably devised, and most laboriously conducted, now showed that the presence of two metals was not required to provide the electricity. One metal, and one other substance separated from it, with a fluid acting upon either the metal or the substance; or a metal separating two fluids, one of which acts upon it; nay, one metal exposed to the same fluid, but acted upon differently on its different sides or surfaces by the fluid's strength differing on the different sides; or one and the same metal in different pieces plunged into the same fluid, at an interval of time—were all found to be combinations which gave the galvanic (or voltaic) shock, the same in kind, though varying in strength. In all these cases, and in every production of electricity by the voltaic process, the chemical action of a fluid upon the metallic substance was a necessary concomitant of the operation.*

During the five following years Davy continued his experiments; and in the autumn of 1806 he communicated to the Royal Society his discovery of the connexion between the different ends of the electric circle and the different component parts of bodies submitted to the action of the fluid. Nothing could be more singular and unexpected than the laws which he now found to regulate this operation, nor anything which promised more clearly a rich harvest of new discoveries. The effect of the current, whether of common or galvanic electricity, in decomposing substances through

* Subsequent experiments have shown that the effect may be produced by other than metallic, or even carbonaceous bodies.

which it passed, had been before known. Thus water had been resolved into its two elements by the passing of the fluid through wires whose points were opposite to each other at a small distance. Nicholson had first made this happy application of the voltaic pile; but he and others had been much disturbed by finding other substances produced as well as oxygen and hydrogen gases. This perplexing circumstance was carefully investigated by Davy; and he showed by a masterly course of experiments, that these substances owed their origin entirely to impurities in the water. When it was quite pure, they wholly disappeared. But he now proceeded farther, and found that when the electric current is thus passed, there is always a separation operated differently at the negative and at the positive part of the current. The oxygen of the water, for example, was accumulated round the positive wire; its hydrogen round the negative. So when a neutral salt was subjected to the process, its acid was evolved round the positive; its alkaline base round the negative wire. The same thing happened when a metallic oxide was operated upon; its oxygen went to the positive, its metallic base to the negative side. The oxygen, or the acid with the oxygen, went to the former; the particles of the base were transferred to the latter, along with the hydrogen of the water in which the solution was made. But a still more extraordinary phenomenon was observed. If there was a liquid interposed between the two poles and the body to be decomposed, the acid, or the oxygen, was found to pass through that interposed liquid to the positive pole, the hydrogen and the matter of the base to the negative pole, and without acting upon the substance of the interposed liquid. Thus suppose a vegetable colour tinging the water in an intermediate cup, acid will pass through it without reddening it, and alkali without making it green. Nay, an acid will pass through an alkaline solution, or an alkali through an acid,

without uniting in either case to form a neutral salt, unless the neutral compound is insoluble, for in that case it falls to the bottom. But muriatic acid will pass through a solution of potash, having been carried over from a solution of common sea salt by the electrical current, or soda will pass through muriatic acid in the same circumstances, without forming in the former case nitrate of potash, or in the latter nitrate of soda. It was also found that the exception in the case of insoluble compounds arises from the mechanical effect of their insolubility, their falling to the bottom; for if supported, as it were, on threads of any convenient substance passing through the intermediate liquid in the line of the electric current, the acid or alkali will pass through that liquid. Thus films of asbestos conducting the electric stream, enabled magnesia or lime to pass; and so were the particles of metal carried over when separated by the operation from nitrate of silver.

It thus appeared certain that an indissoluble connexion exists between chemical and electric action, if indeed it was not even proved that chemical affinity and electricity are identical. The science of Electro-Chemistry, at all events, now arose out of Davy's discoveries, and he is entitled to be regarded as its founder.

It may easily be conceived that these important truths excited generally the anxious attention of philosophers. The French National Institute, greatly to their honour, though the war between the two countries never raged more fiercely than now, and France never reached a higher pitch of military glory, crowned Davy with the first honour founded by Napoleon for scientific desert. But it was even more honourable to the philosopher, that great as his discoveries had been, expectation was high of the still more important results which must soon come from the discovery of so new a law of electrical and chemical action. I can well remember that we used in discussing the subject to look

forward with perfect confidence to the analysis of the bodies which had hitherto proved the most stubborn, and expected soon to find the fixed alkalis, and even the alkaline earths, shown to be oxides, as by some very imperfect experiments they had at one time been supposed to be proved, when it was ascertained that the metallic buttons found at the bottom of the crucible in which their reduction had been attempted by carbonaceous or phosphoric re-agents had come from the black lead in the pot. Nor must we omit to mention the truly candid and magnanimous proceeding of Davy, so worthy of a philosopher, in making public, with the fullest details, his proceedings, by which it was manifest he intended still to persevere till he should make other discoveries. Any one possessed of a strong battery, deeply reflecting on the paper of autumn 1806, and perceiving that the positive wire had such a strong attraction for oxygen as to take it from metallic oxides, reducing them to their reguline state, might well have bethought him of subjecting the alkalis to his machine; and he would then have had the fame, though, in truth, Davy would have had the merit, of the grand discovery.

That discovery was not long delayed. About a year after the former, that is in October 1807, after in vain endeavouring to decompose the alkalis when mixed with water, for he then only could decompose that fluid, he exposed them in the dry state; that is, made liquid by fusion, without any other substance but heat to dissolve them—and, to his great delight, he found, as he had a right to expect, that the process of deoxidation proceeded by the positive wire attracting the oxygen, while globules of a metallic substance were found at the negative wire. The great attraction of this metal for oxygen made it impossible to keep it either in the air or in water. It burnt spontaneously in the air and became alkali—it decomposed water in like manner, and formed an alkaline solution. The

two fixed alkalis both yielded in this process metallic bases; but that of potash had alone the quality of combustion at the temperature of 150°, and it was, though a metal, lighter than water in the proportion of 97 to 100. When thrown into water in the air, it detonates and burns with violence, forming a solution of potash. The metal from soda is still lighter, being to water as 86 to 100; but it does not so easily unite with oxygen, though it decomposes water with a hissing noise, and makes with it a solution of soda. To these metals the discoverer gave the name of *potassium* and *sodium*. The glory of having now made the greatest discovery of the age was plainly Davy's; and it was not the result of happy accident, but of laborious investigation, conducted with a skill and a patience equally admirable, and according to the strict rules of the soundest philosophy. He had indeed begun by discovering the laws of electrical action, and had thus formed the means of his new discovery, which was the fruit of the science he had founded, as Newton's theory of dynamics and of astronomy was the fruit of the calculus which he had so marvellously discovered when hardly arrived at man's estate.

The wonder excited by the strange bodies with which philosophers were thus brought acquainted, was of course in part owing to their novel and singular properties, which formed no part of the discoverer's merits, yet might be reckoned as the perquisites of his genius. His praise would have been the same if instead of at once discovering the alkalis to be oxides, and the metal forming the base to be one lighter than water, or bees'-wax or box-wood, and the other to burn unheated in the open air, he had only shown those salts to be oxides of well-known metals. Yet, as his investigation had been crowned with the discovery of strange substances, metallic, and yet like no other metals, we justly admire the more, and the more thank him for his double service rendered to science.

The long labour thus ending in so mighty a result, and the excitement naturally enough produced in an irritable habit, threw him into an illness of a most serious complexion. For many days he lay between life and death in a low nervous fever, and it was not till the following March that he could resume his inquiries into the composition of the alkaline earths. It is to the credit of chemists that no one deemed himself at liberty to interfere with him, as any one might now by only following his footsteps have done, and thus analysed these earthy bodies. He himself, early in the summer following his illness, had reduced lime, magnesia, strontites, and barytes. In these experiments he was greatly assisted by the ingenious contrivances which Gay-Lussac and Thenard had recently used for the reduction of the alkaline oxides. The metals thus discovered were not any wise light or fusible like potassium and sodium; but they burnt with a bright light on being exposed to considerable degrees of heat, and they decomposed water; and either by their combustion, or their exhibition to water, they reproduced the alkaline earths.

A number of other experimental researches led Davy to new and curious observations on the constitution and habits of different substances. But we need only mention the most important of these, for it was a discovery very unexpected both by himself and the chemical world at large. The acid hitherto called oxygenated muriatic, or oxymuriatic, on account of its powerful acid qualities, had been always from thence supposed to contain an excess of oxygen, believed to be the acidifying principle. At last Gay-Lussac and Thenard, in 1809, concluded from some experimental researches, or rather they suspected, that it might be a simple and elementary substance; but they on the whole still inclined to think it contained oxygen according to the old and received opinion. Davy now found, by a course of satisfactory experiments which have fixed the

opinions of all philosophers on the subject, that the suspicion of those eminent men was well founded; that the oxymuriatic acid is a simple substance, containing no oxygen, that it unites with oxygen to form an acid, which forms with alkalis the detonating salts hitherto called oxymuriates, as being supposed to contain oxymuriatic acid combined with alkaline bases; and finally, that with hydrogen it forms the acid long and well known as the muriatic or marine. To the oxymuriatic acid he gave the name of *chlorine* from its green colour, and to common muriatic acid that of *hydrochlorine.* The union of chlorine and oxygen he calls *chlorine* acid, and its compounds, of course, *chlorates.* This is justly reckoned one of the most important of Davy's many brilliant discoveries.

It remains to make mention of the valuable present which this great philosopher offered to humanity—his safety-lamp. The dreadful ravages made on human life by the fire-damp explosions—that is, the burning of hydrogen gas in mines—had often attracted the notice of both the mine-owner and the philanthropist. Various inventions had been fallen upon to give light in those recesses of the earth with so low a degree of heat as should be insufficient to explode the gas. One of them was a series of flints playing by machinery against each other so as to give a dim light; but this had very little success; it was clumsy, and it was not effectual so as to cause its use by miners. The ventilation of the galleries by furnaces and even by air-pumps was chiefly relied on as a preventive; but gas would collect in spite of all preventives, and the destruction of a hundred or more lives was not an unusual calamity. Davy about the year 1815 turned his attention to the subject, and after fully ascertaining that carburetted hydrogen is the cause of the fire-damp, and finding in what proportions it must be mixed with air in order to explode (between six and fourteen times its bulk), he was surprised to observe, in the course of his experi-

ments made for the purpose of ascertaining how the inflammation takes place, that the flames will not pass through tubes of a certain length or smallness of bore. He then found that if the length be diminished, and the bore also reduced, the flames will not pass; and he further found that by multiplying the number of the tubes, their length may safely be diminished to hardly anything, provided their bore be proportionably lessened. Hence it appeared that gauze of wire, whose meshes were only one twenty-second of an inch diameter, stopped the flame, and prevented the explosion. The candle or lamp being wrapt in such gauze, and all access to the external air prevented except through the meshes, it is found that the lamp may be safely introduced into a gallery filled with fire-damp; a feeble blue flame will take place inside the gauze, but no explosion, even if the wire be heated nearly red.

The theory is, but it seems very questionable, that the conducting power of the wire carrying off the heat prevents a sufficient quantity reaching the explosive compound. Subsequent inquiries seem to prove that although in a still atmosphere of explosive gas the lamp is a perfect protection, yet it does not prevent a current of gas from penetrating to the flame and exploding. It is attempted to guard against this by interposing a tin shield or screen; but a current very often in mining operations arises before any notice can be given. Had Davy's life and health been prolonged, he might have further improved his invention so as to meet this objection. He certainly never was fully convinced of its force, as I know from having discussed the subject with him; and no doubt the testimony of so great an engineer as the late Mr. Buddle, given before a Parliamentary Committee to whom the examination of this important subject was referred, deserves great attention. He positively affirmed that "having seen 1000, and sometimes 1500 safety-lamps in daily use, and in all possible varieties of explosive mixtures, he had never

known one solitary instance of an explosion." As for the lamentable accidents which continue to happen, we can scarcely doubt that they originate in the dreadful carelessness of their own and of other men's lives, which seems to be engendered in those who are habitually exposed to great danger. That they themselves are the first to suffer for it, can only suppress the outward expression of the feelings which recklessness like this is fitted to produce.

It redounds to the credit of the north country mine-owners that in 1817 they invited the inventor of the Lamp to a public entertainment, and presented him with a service of plate of two thousand pounds value. It must be remembered that he had generously given to the public the whole benefit of his invention, and thus sacrificed the ample profit which a patent must have enabled him to acquire for himself.

Davy had as early as 1806 been chosen a foreign associate of the French Institute. In 1812 he received from the Regent the honour of knighthood. About the same time he married Mrs. Apreece, a lady whose ample fortune was by far the least valuable part of her accomplishments—a person of great virtue, admirable talents, and extensive information. Of this marriage there has been no issue. In October, 1813, he published his 'Elements of Chemical Philosophy,'—a hasty and even somewhat crude work, but abounding, as whatever he wrote was sure to abound, in important and ingenious observations. In the following year appeared his 'Elements of Agricultural Chemistry,' of which the same general character may be given. In 1816 he was created a baronet.

Napoleon had, during the war, given him permission to visit the extinguished volcanoes in Dauvergne, and to pass through France towards Naples, Vesuvius being then in a state of eruption. His reception at Paris was very warm, but unfortunately he failed to retain the affection of his colleagues in the Institute.

Their complaint against him for having interfered, as they termed it, with their recent discovery of iodine, on which, having obtained a specimen, he chose, naturally enough, to make experiments, appears incomparably absurd. He had never complained of their interference, during his illness in 1807, with the process of deoxygenation by means of galvanic action; on the contrary, he had availed himself thankfully of the lights shed by their ingenuity on his process, and had immediately after made new discoveries, at which they had failed to arrive. It may be more true that his manners were unpleasing; and, as ever happens when a great man is also a shy one, he was charged with being supercilious and cold. They who knew him will at once acquit him of any such charge; but he was painfully timid by nature when mixing with society; and hence the mistake of our neighbours, who, though great critics in manner, are far from being infallible, and are exceedingly susceptible—fully as susceptible as he was shy. Possibly they looked down upon him in consequence of a peculiarity which he no doubt had. He was fond of poetry, and an ardent admirer of beauty in natural scenery. But of beauty in the arts he was nearly insensible. They used to say in Paris that on seeing the Louvre, he exclaimed that one of its statues was "a beautiful stalactite;" and it is possible that this callousness, or this jest, whichever it might be, excited the scorn or the humour of men not more sincere lovers of sculpture than himself, or more able judges of its merits, but better disposed to conceal their want of taste or want of skill.

When Sir Joseph Banks terminated his long and respectable course in 1820, Davy was unanimously chosen to succeed him as President of the Royal Society, and continued to fill that distinguished office until, his health having failed, he resigned it in 1827, and was succeeded by his early patron Davies Giddy Towards the end of 1825 he had an apoplectic seizure, which, though slight (if any such attack can be so

called), left a paralytic weakness behind, and he was ordered to go abroad in search of a milder and dryer climate. He returned home in the following autumn, not very ill, but not much restored in strength, and unable to continue his scientific labours. The work on fly-fishing called 'Salmonia' was the amusement of those hours in which, comparatively feeble, his mind yet exerted what energy remained to it, on the favourite pursuit of his leisure. It contains both curious information on natural history, and many passages of lively and even poetical description. The same may be said of many things in his latest work, 'Last Days of a Philosopher,' which he wrote in the year after, when he again went to the continent in search of health. He wintered at Rome, and in May 1829, on his arrival at Geneva, after passing the day in excellent spirits, and dining heartily on fish, he had a fatal apoplectic attack in the night, and died early in the next morning, 29th May, without a struggle.

There needs no further remark, no general character to present a portrait of this eminent individual. Whoever has perused the history of his great exploits in science, with a due knowledge of the subject, has already discerned his place, highest among all the great discoverers of his time. Even he who has little acquaintance with the subjects of his labours may easily perceive how brilliant a reputation he must have enjoyed, and how justly; while he who can draw no such inference from the facts would fail to obtain any knowledge of Davy's excellence from all the panegyrics with which general description could encircle his name.*

* It may not be impertinent to relate here a singular proof of the admiration in which his name was held by his countrymen, and how well it became known even among the common people. Retiring home one evening he observed an ordinary man showing the moon and a planet through a telescope placed upon the pavement. He went up and paid his pence for a look. But no such thing would they permit. "That's Sir Humphry," ran among the people; and the exhibitor, returning his money, said, with an important air which exceedingly delighted him, that he could not think of taking anything from a brother philosopher.

SIMSON.

The wonderful progress that has been made in the pure mathematics since the application of algebra to geometry, begun by Vieta in the sixteenth, completed by Des Cartes in the seventeenth century, and especially the still more marvellous extension of analytical science by Newton and his followers, since the invention of the Calculus, has, for the last hundred years and more, cast into the shade the methods of investigation which preceded those now in such general use, and so well adapted to afford facilities unknown while mathematicians only possessed a less perfect instrument of investigation. It is nevertheless to be observed that the older method possessed qualities of extraordinary value. It enabled us to investigate some kinds of propositions to which algebraic reasoning is little applicable; it always had an elegance peculiarly its own; it exhibited at each step the course which the reasoning followed, instead of concealing that course till the result came out; it exercised the faculties more severely, because it was less mechanical than the operations of the analyst. That it afforded evidence of a higher character, more rigorous in its nature than that on which algebraic reasoning rests, cannot with any correctness be affirmed; both are equally strict; indeed if each be mathematical in its nature, and consist of a series of identical propositions arising one out of another, neither can be less perfect than the other, for of certainty there can be no degrees. Nevertheless it must be a matter of regret—and here the great master and author of modern mathematics has joined

in expressing it—that so much less attention is now paid to the Ancient Geometry than its beauty and clearness deserve; and if he could justly make this complaint a century and a half ago, when the old method had but recently, and only in part, fallen into neglect and disuse, how much more are such regrets natural in our day, when the very name of the Ancient Analysis has almost ceased to be known, and the beauties of the Greek Geometry are entirely veiled from the mathematician's eyes! It becomes, for this reason, necessary that the life of Simson, the great restorer of that geometry, should be prefaced by some remarks upon the nature of the science, in order that, in giving an account of his works, we may say his discoveries, it may not appear that we are recording the services of a great man to some science different from the mathematical.

The analysis of the Greek geometers was a method of investigation of peculiar elegance, and of no inconsiderable power. It consisted in supposing the thing as already done, the problem solved, or the truth of the theorem established; and from thence it reasoned until something was found, some point reached, by pursuing steps each one of which led to the next, and by only assuming things which were already known, having been ascertained by former discoveries. The thing thus found, the point reached, was the discovery of something which could by known methods be performed, or of something which, if not self-evident, was already by former discovery proved to be true; and in the one case a construction was thus found by which the problem was solved, in the other a proof was obtained that the theorem was true, because in both cases the ultimate point had been reached by strictly legitimate reasoning, from the assumption that the problem had been solved, or the assumption that the theorem was true Thus, if it were required from a given point in a straight line given by position, to draw a straight

line which should be cut by a given circle in segments, whose rectangle was equal to that of the segments of the diameter perpendicular to the given line—the thing is supposed to be done; and the equality of the rectangles gives a proportion between the segments of the two lines, such that, joining the point supposed to be found, but not found, with the extremity of the diameter, the angle of that line with the line sought but not found, is shown by similar triangles to be a right angle, *i. e.*, the angle in a semicircle. Therefore the point through which the line must be drawn is the point at which the perpendicular cuts the given circle. Then, suppose the point given through which the line is to be drawn, if we find that the curve in which the other points are situate is a circle, we have a local theorem, affirming that, if lines be drawn through any point to a line perpendicular to the diameter, the rectangle made by the segments of all the lines cutting the perpendicular is constant; and this theorem would be demonstrated by supposing the thing true, and thus reasoning till we find that the angle in a semicircle is a right angle, a known truth. Lastly, suppose we change the hypothesis, and leave out the position of the point as given, and inquire after the point in the given straight line from which a line being drawn through a point to be found in the circle, the segments will contain a rectangle equal to the rectangle under the perpendicular segments—we find that one point answers this condition, but also that the problem becomes indeterminate; for every line drawn through that point to every point in the given straight line has segments, whose rectangle is equal to that under the segments of the perpendicular. The enunciation of this truth, of this possibility of finding such a point in the circle, is a *Porism*. The Greek geometers of the more modern school, or lower age, defined a Porism to be a proposition differing from a local theorem by a defect or defalcation in the hypothesis;

and accordingly we find that this porism is derived from the local theorem formerly given, by leaving out part of the hypothesis. But we shall afterwards have occasion to observe that this is an illogical and imperfect definition, not coextensive with the thing defined; the above proposition, however, answers every definition of a Porism.

The demonstration of the theorem or of the construction obtained by investigation in this manner of proceeding, is called *synthesis*, or *composition*, in opposition to the *analysis*, or the process of investigation; and it is frequently said that Plato imported the whole system in the visits which he made, like Thales of Miletus and Pythagoras, to study under the Egyptian geometers, and afterwards to converse with Theodorus at Cyrene, and the Pythagorean School in Italy. But it can hardly be supposed that all the preceding geometers had worked their problems and theorems at random; that Thales and Pythagoras with their disciples, a century and a half before Plato, and Hippocrates, half a century before his time, had no knowledge of the analytical method, and pursued no systematic plan in their researches, devoted as their age was to geometrical studies. Plato may have improved and further systematized the method, as he was no doubt deeply impressed with the paramount importance of geometry, and even inscribed upon the gates of the Lyceum a prohibition against any one entering who was ignorant of it. The same spirit of exaggeration which ascribes to him the analytical method, has also given rise to the notion that he was the discoverer of the Conic Sections; a notion which is without any truth and without the least probability.

Of the works written by the Greek geometers some have come down to us; some of the most valuable, as the 'Elements' and 'Data' of Euclid, and the 'Conics' of Apollonius. Others are lost; but, happily, Pappus, a mathematician of some merit, who flourished in the

Alexandrian school about the end of the fourth century, has left a valuable account of the geometrical writings of the elder Greeks. His work is of a miscellaneous nature, as its name, 'Mathematical Collections,' implies; and excepting a few passages, it has never been published in the original Greek. Commandini, of Urbino, made a translation of the whole six books then discovered; the first has never been found, but half the second being in the Savilian library at Oxford, was translated by Wallis a century later. Commandini's translation, with his learned commentary, was not printed before his death, but the Duke of Urbino (Francesco Maria) caused it to be published in 1588, at Pisa, and a second edition was published at Venice the next year: a fact most honourable to that learned and accomplished age, when we recollect how many years Newton's immortal work was published before it reached a second edition, and that in the seventeenth and eighteenth centuries.

The two first books of Pappus appear to have been purely arithmetical, so that their loss is little to be lamented. The eighth is on mechanics, and the other five are geometrical. The most interesting portion is the seventh; the introduction of which, addressed to his son as a guide of his geometrical studies, contains a full enumeration of the works written by the Greek geometers, and an account of the particular subjects which each treated, in some instances giving a summary of the propositions themselves with more or less obscurity, but always with great brevity. Among them was a work which excited great interest, and for a long time baffled the conjectures of mathematicians, Euclid's three books of 'Porisms:' of these we shall afterwards have occasion to speak more fully. His 'Loci ad Superficiem,' apparently treating of curves of double curvature, is another, the loss of which was greatly lamented, the more because Pappus has given no account of its contents. This he had done in the

case of the 'Loci Plani' of Apollonius. Euclid's four books on conic sections are also lost; but of Apollonius's eight books on the same subject, the most important of the whole series, the 'Elements' excepted, four were preserved, and three more were discovered in the seventeenth century. His Inclinations, his Tactions or Tangencies, his sections of Space and of Ratio, and his Determinate section, however curious, are of less importance; all of them are lost.

For many years Commandini's publication of the 'Collections' and his commentary did not lead to any attempt at restoring the lost works from the general account given by Pappus. Albert Girard, in 1634, informs us in a note to an edition to Stevinus, that he had restored Euclid's 'Porisms,' a thing eminently unlikely, as he never published any part of his restoration, and it was not found after his decease. In 1637, Fermat restored the 'Loci Plani' of Apollonius, but in a manner so little according to the ancient analysis, that we cannot be said to approach by means of his labours the lost book on this subject. In 1615, De la Hire, a lover and a successful cultivator of the ancient method, published his Conic Sections, but synthetically treated; he added afterwards other works on epicycloids and conchoids, treated on the analytical plan. L'Hôpital, at the end of the seventeenth century, published an excellent treatise on Conics, but purely algebraical. At the beginning of the eighteenth century, Viviani and Grandi applied themselves to the ancient geometry; and the former gave a conjectural restoration (Divinatio) of Aristæus's 'Loci Solidi,' the curves of the second or Conic order. But all these attempts were exceedingly unsuccessful, and the world was left in the dark, for the most part, on the highly interesting subject of the Greek geometry. We shall presently see that both Fermat and Halley, its most successful students, had made but an inconsiderable progress in the most difficult branches.

How entirely the academicians of France were either careless of those matters, or ignorant, or both, appears by the 'Encyclopédie'; the mathematical department of which was under no less a geometrician than D'Alembert. The definition there given of analysis makes it synonymous with algebra: and yet mention is made of the ancient writers on analysis, and of the introduction to the seventh book of Pappus, with only this remark, that those authors differ much from the modern analysts. But the article 'Arithmetic' (vol. i., p. 677) demonstrates this ignorance completely; and that Pappus's celebrated introduction had been referred to by one who never read it. We there find it said, that Plato is supposed to have invented the ancient analysis; that Euclid, Apollonius, and others, including Pappus himself, studied it, but that we are quite ignorant of what it was: only that it is by some conceived to have resembled our algebra, as else Archimedes could never have made his great geometrical discoveries. It is, certainly, quite incredible that such a name as D'Alembert's should be found affixed to this statement, which the mere reading of any one page of Pappus's books must have shown to be wholly erroneous; and our wonder is the greater, inasmuch as Simson's admirable restoration of Apollonius's 'Loci Plani' had been published five years before the 'Encyclopédie' appeared.

Again, in the 'Encyclopédie,' the word Analysis, as meaning the Greek method, and not algebra, is not even to be found. Nor do the words synthesis, or composition, inclinations, tactions or tangencies, occur at all; and though Porisms are mentioned, it is only to show the same ignorance of the subject: for that word is said to be synonymous with 'lemma,' because it is sometimes used by Pappus in the sense of subsidiary proposition.* When Clairault wrote his inestimable work on curves of double curvature, he made no reference whatever to Euclid's 'Loci ad Superficiem,'

* Euclid uses the word Corollary in his Elements.

much less did he handle the subject after the same manner; he deals, indeed, with matters beyond the reach of the Greek geometry.

Such was the state of this science when Robert Simson first applied to it his genius, equally vigorous and undaunted, with the taste which he had early imbibed for the beauty, the simplicity, and the closeness of the ancient analysis.

ROBERT SIMSON was born on the 14th October (O.S.), 1687, at Kirton Hill, in the parish of Wester Kilbride, in Ayrshire. His father, John Simson, was a merchant in Glasgow; his grandfather, Patrick, was minister of Renfrew, and Dean of the Faculties in the University of Glasgow. Having been deprived at the Restoration, on being reinstated at the Revolution, he accompanied Principal Carstairs and a deputation as one of the Commissioners from the Kirk of Scotland to address the Sovereigns. Being a man of fine presence, it is related that the Queen and her maids of honour mistook him for the Principal, till the King set them right by presenting Carstairs to them. The grandson, Robert, is said to have been the eldest of seventeen children; and the estate of Kirton Hill, which had been in the family for several generations, being inconsiderable, it was necessary for him, as well as his brothers, to be placed in some profession. The assertion is made in one account, written by a son of Professor Millar, and is likely to be correct, that he was intended for the medical profession, and being sent to Leyden studied under Boerhaave. He appears to have been at first intended for the Church, and to have changed his plan. Dr. Traill, however, says, that he was always intended for the Church, and that when the University of St. Andrew's, in 1746, wished to confer on him a degree, they made him a Doctor of Medicine, because he had studied botany in his youth. Nothing can be more improbable than this story; for

to give him a degree they had only to make him Doctor of Laws, instead of taking a step which for ever threw discredit upon their medical honours. Mr. Millar must have heard the truth from his father and the other professors, who had the honour of knowing Dr. Simson personally, and never could have imagined or invented the circumstance of his studying under Boerhaave.*

Of his early years we know little; but that he was always extremely fond of reading is certain; and he must have had a considerable turn for mechanical pursuits if the tradition in the neighbourhood of Kirton Hill be well founded, which ascribes to him the making, or at least designing and placing a dial of a curious form (which I have seen) on a neatly ornamented pedestal in the garden of his father's house. At the usual early age of matriculation in Scotland, he was sent to the University of Glasgow, and he had there made considerable progress in his studies before the love of mathematical pursuits appeared to possess him. His attention was directed to theology, to logic, to Oriental learning; and in the latter he had made such progress, that a relation who taught the class having fallen ill, Simson easily supplied his place for part of a session, the Scottish academical year. It was while engaged in theological studies that the mathematics first seized hold of his mind. He used in after life to relate how, wearied with the controversies to which his clerical studies led him, he would refresh himself with philosophical reading; and not seldom finding himself there also tossed about by conflicting dogmas, he retired for peace and shelter to the certain science of necessary truth; "and then," said he, "I always found myself refreshed with rest."

* The account which I have seen was in the late Earl of Buchan's possession, and was extended by matters collected when he himself studied at Glasgow. It seems by the mathematical appearance of it to have come from James Millar, himself one of the professors.

It happened that no lecture or teaching of any kind was given by the professor who filled the mathematical chair, receiving its emoluments, and neglecting its duties, when Simson went to the University. But curiosity, a propensity ever strong in his nature through his whole life, made him wish to see what the science was, and he borrowed from a friend a copy of Euclid, the work which he was destined afterwards to give forth in a perfection that has made all other editions of that great classic be forgotten. Over the elements of the science he pored assiduously and alone, with only the aid of suggestions occasionally given by a student some years older than himself; and the study falling in with his genius and his taste, he soon made himself master of the first six books, comprising plain geometry, and the eleventh and twelfth, treating of solids, those at least which are bounded by planes or by circular arches. But he did not neglect the other branches of science taught at the College; and he also gave his attention to the literary parts of education, so well mastering the Latin and Greek languages as to become a learned and accurate scholar. It was in the mathematics, however, that he chiefly excelled; and his accomplishments in that science becoming known to the professorial body (the Senatus Academicus), in whom is vested the patronage of the mathematical chair, and an early vacancy being foreseen, they offered him the succession in that event. Being then in his twenty-second year, he modestly declined to undertake so important a charge, but requested a year's delay, during which he might repair to London, and become more familiar with the science and its cultivators. We may hence perceive that there could then have been no one at all versed in the mathematics at Glasgow; and the allowing so important a branch of science to remain for so many years untaught because the teacher who received the ample emoluments of the chair either could not or would not perform its duties, affords a

sufficient commentary upon the great abuse likely to flow from vesting the patronage of a professorship in the colleagues of the teacher. I have known a professor's son appointed to the same chair, with few or no mathematical acquirements, because his father was much and justly respected among the members of the academical body. The same thing could not happen in Edinburgh, where the Crown or the magistrates have the patronage of all the professorships excepting one, and that is in the representative of the founder.*

Simson repaired accordingly to London, where he became intimately acquainted, among others, with Jones the optician, with Henry Ditton of Christ's Hospital, under whose tuition he placed himself, with Carswell, above all, with Edmund Halley, then a captain in the Navy, afterwards so celebrated as Dr. Halley; of whom he used to assert that "he had never known any other man of so acute and penetrating an understanding, and of so pure a taste." From him he received much personal kindness, and what he had reason to value still more, the advice to prosecute his study of the Ancient Geometry, and attempt restoring its lost books. Halley made him a present of his copy of Pappus, with notes in his own hand. But though these accidental circumstances tended to direct his attention towards the scrupulous rigour as well as surpassing elegance of the Greek methods, it is a great mistake to suppose that he objected to the strictness of the modern analysis as inadequate. That he deemed its beauty inferior, and that he was right in so deeming, is certain; but that he questioned the solidity of its foundations is wholly untrue. Not only did he always explain its principles to his pupils, though in a manner peculiar to himself, but he has left behind him a treatise demonstrating the fundamental laws of the calculus, and we now possess it in a printed form.

* Agriculture, in the Pulteney Family.

Equally groundless is the notion that he questioned the soundness of the Newtonian Philosophy. He was not enabled to make Sir Isaac's acquaintance during his residence in London; but among those he lived with he constantly had seen him viewed with a peculiar observance, and Halley in particular regarded him as hardly human, and his attainments in science as exalting our species, while they ennobled himself, its rarest individual. Simson's copy of the 'Principia' is fully noted in the margin with illustrations, showing that he entirely assented to the results of the investigations in the several propositions, and only wished to substitute certain steps in the demonstrations. Professor Robison has also related (Art. Simson, Encyc. Brit. xvii. 505) his constant remark, that the celebrated proposition in the 'Principia' on inverse centripetal forces "was the most important ever delivered to mankind in the mixed mathematics."

While he remained in London the expected vacancy occurred in the chair at Glasgow, and he returned thither. The professors appear to have thought it right that their former neglect of duty should be compensated by a very superfluous show of more than needful attention to it on this occasion; for they required Mr. Simson to give proof of his fitness to succeed the sinecure incumbent, by solving a geometrical problem, of which it is all but absolutely certain that they could have no knowledge, unless the question was so simple as to afford no test of the candidate's capacity. He produced, however, what they might better understand, testimonials from known mathematicians in London, a farther proof of there being no cultivators of the science then resident in the metropolis of Scottish manufactures.

He was thus appointed professor in 1711, and immediately began the regular course of instruction, which he continued for half a century. He taught two classes five days a week for seven months every

year. Though geometry was his own favourite study, he was a thorough algebraist also, and so well versed in mathematical science at large, that he gave lectures on its general history. With astronomy, and the other branches of the mixed mathematics, he was no less conversant; and in various departments of physics he had made great progress. In botany he was particularly expert; it formed his chosen amusement during the walks in which he relaxed from his severer studies. His curiosity led him into other paths of science. To logic, that of the schools, he had given so much attention, that of a tract, composed by him upon its principles, some portion remains among his papers; it is said to possess great merit; and doubtless this study was congenial to the one which he mainly pursued, nor could it fail to aid his strict and luminous method of both defining, demonstrating, and explaining the truths of geometry.

Among his colleagues, after he had been professor a few years, were some of the most eminent men of that, or indeed of any age. Moore, professor of Greek, and author of the admirable and elegant 'Grammar;' Hutcheson, and Adam Smith, successively teachers of moral philosophy; Cullen, the celebrated physician; Black, the great founder of modern chemistry—all taught while Simson flourished; Millar only became professor of law at the close of the brilliant period now referred to, and Robison succeeded Black in 1761, soon after Simson's resignation.

But a teacher's influence is nothing in surrounding himself with illustrious colleagues; of great pupils he may more easily obtain a following. Of these, Dr. Simson had some whose names are still honoured among mathematicians. Williamson, a favourite pupil, and a man of great promise, whose early death at the Factory of Lisbon, to which he was chaplain, alone prevented him from following with distinction his master's footsteps; Scott, preceptor to

George III. when Prince of Wales, afterwards a Commissioner of Excise in London, perhaps the most accomplished of all amateur mathematicians who never gave their works to the world; Traill, author of the excellent elementary treatise of algebra, of a very learned and exceedingly ill-written, indeed, hardly readable, life of his friend and teacher, but a man of great capacity for science, entirely extinguished, together with his taste for its pursuits (as Professor Playfair used to lament), by the sinecure emoluments of the Irish Church; but above all, Matthew Stewart, Simson's favourite pupil, and whose suggestions, and indeed contributions, he records in his works with appropriate eulogy, as he does on one occasion an ingenious theorem of Traill—these were among his scholars, and were, with Robison, the most distinguished of their number. His method of lecturing is, by both of the pupils who have written his history, Professor Robison and Dr. Traill, described as singularly attractive. His explanations were perfectly clear, and were delivered with great spirit, as well as with the pure taste which presided over all his mathematical processes. His elocution was distinct and natural, his whole manner at once easy and impressive. He did not confine his tuition to the chair, but encouraged his pupils to propound their difficulties in private, and was always accessible to their demands of assistance and advice. Hence the affectionate zeal with which they followed his teaching and ever cherished his memory.

Successful, however, as he proved in the chair, his genius was bent to the diligent investigation of truth, in the science of which he was so great a master. The ancient geometry, that of the Greeks of which I have spoken, early fixed his attention and occupied his mind by its extraordinary elegance, by the lucid clearness with which its investigations are conducted, by the exercise which it affords to the reasoning faculties,

and above all, by the absolute rigour of its demonstrations. He never undervalued modern analysis; it is a great mistake to represent him as either disliking its process, or insensible to its vast importance for the solution of questions which the Greek analysis is wholly incapable of reaching. But he considered it as only to be used in its proper sphere; and that sphere he held to exclude whatever of geometrical investigation can be, with convenience and elegance, carried on by purely geometrical methods. The application of algebra to geometry, it would be ridiculous to suppose that either he or his celebrated pupil Stewart disliked or undervalued. That application forms the most valuable service which modern analysis has rendered to science. But they did object, and most reasonably and consistently, to the introduction of algebraic reasoning wherever the investigation could, though less easily, yet far more satisfactorily, be performed geometrically. They saw, too, that in many instances the algebraic solution leads to constructions of the most complex, clumsy, unmanageable kind, and therefore must be, in all these instances, reckoned more difficult, and even more prolix than the geometrical, from the former being confined to the expression of all the relations of space and position, by magnitudes, by quantity and number, (even after the arithmetic of sines had been introduced,) while the latter could avail itself of circles and angles directly. They would have equally objected to carrying geometrical reasoning into the fields peculiarly appropriate to modern analysis; and if one of them, Stewart, did endeavour to investigate by the ancient geometry physical problems supposed to be placed beyond its reach—as the sun's distance, in which he failed, and Kepler's problem, in which he marvellously succeeded, that of dividing the elliptical area in a given ratio by a straight line drawn from one focus—this is to be taken only as an homage to the undervalued potency

of the Greek analysis, or at most, as a feat of geometrical force, and by no means as an indication of any wish to substitute so imperfect, however beautiful, an instrument, for the more powerful, though more ordinary one of the calculus which "alone can work great marvels." At the same time, and with all the necessary confession of the merits of the modern method, it is certain that those geometricians would have regarded the course taken by some of its votaries in more recent times as exceptionable, whether with a view to clearness or to good taste: a course to the full as objectionable as would be the banishing of algebraical and substituting of geometrical symbols in the investigations of the higher geometry. La Place's great work, the 'Mécanique Céleste,' and La Grange's 'Mécanique Analytique,' have treated of the whole science of dynamics and of physical astronomy, comprehending all the doctrine of trajectories, dealing with geometrical ideas throughout, and ideas so purely geometrical that the algebraic symbols, as far as their works are concerned, have no possible meaning apart from lines, angles, surfaces; and yet in their whole compass they have not one single diagram of any kind. Surely, we may ask if $\frac{y}{dy}\sqrt{dx^2+dy^2}$, $\frac{ds^3}{dx^2 d\left(\frac{dy}{dx}\right)}$ * can possibly bear any other meaning than the tangent and the radius of curvature of a curve line: that is, a straight line touching a curve, and a circle whose curvature is that of another curve where they meet; any meaning, at least, which can make it material that they should ever be seen on the page of the analyst. These expressions are utterly without sense, except in reference to geometrical considerations; for although x and

* Or $\frac{(dx^2+dy^2)^{\frac{3}{2}}}{dx^2 d\left(\frac{dy}{dx}\right)}$

y are so general that they express any numbers, any lines, nay, any ideas, any rewards or punishments, any thoughts of the mind, it is manifest that the square of the differential of a thought, or the differential of the differential of a reward or punishment, has no meaning; and so of every thing else but of the very tangent or the osculating circle's radius: consequently the generality of the symbols is wholly useless; the particular case of two lines being the only thing to which the expressions can possibly be meant to apply. Why, then, all geometrical symbols should be so carefully avoided when we are really treating of geometrical examples and geometrical ideas, and of these alone, seems hard to understand.

As the exclusive lovers of modern analysis have frequently and very erroneously suspected the ancients of possessing some such instrument, and concealing the use of it by giving their demonstrations synthetically after reaching their conclusions analytically, so some lovers of ancient analysis have supposed that Sir Isaac Newton obtained his solutions by algebraic investigations, and then covered them with a synthetic dress. Among others, Dr. Simson leant to this opinion respecting the 'Principia.' He used to say that he knew this from Halley, by whose urgent advice Sir Isaac was induced to adopt the synthetic form of demonstration, after having discovered the truths analytically. Machin is known to have held the same language; he said that the 'Principia' was algebra in disguise. Assuredly, the probability of this is far greater than that of the ancients having possessed and kept secret the analytical process of modern times. In the preface to his 'Loci Plani,' Dr. Simson fully refutes this notion respecting the ancients: a notion which, among others, no less a writer than Wallis had strongly maintained.*

* Algebra Præf. "Hanc Græcos olim habuisse non est quod dubitemus; sed studio celatam, nec temere propalandam. Ejus effectus (utut

Dr. Simson is by some supposed to have had at one time the intention of discussing at large the proper limits of the ancient and the modern analysis in the investigation of mathematical truths. This no doubt appears to be the meaning of a passage in his preface to the Conic Sections: "In quantum autem differat analysis geometrica ab eâ quæ calculo instituitur algebraico, atque *ubi hæc aut illa sit usurpanda, alias disserendum.*" Professor Robison thought he had seen a portion of the work; but he must have been mistaken; for in answer to Mr. Scott's letter urging him to publish this, and referring to the preface in the words just cited, he expressly says, that though this passage might well mislead, he never meant, except by "blundering in the expression, anything of the kind, had no paper, and never wrote anything about the matter:" and this was written in 1764, four years before his death, and eleven or twelve years after Professor Robison attended his class. Nothing can be more clear than that between 1764 and his death, in 1768, he never attempted any work of moment; much more any work such as the one in question, which we thus have his own authority for saying he never had previously entertained any intention of composing. It is much to be lamented that he never did give such a work to the world. His thoughts had often been very profoundly directed to the subject; and no one was so well fitted to handle it with the learning and with the judgment which its execution required.

clam celatæ) satis conspicui apud Archimedem, Apollonium, aliosque." It is strange that any one of ordinary reflection should have overlooked the utter impossibility of all the geometricians in ancient times keeping the secret of an art which must, if it existed, have been universally known in the mathematical schools, and at a time when every man of the least learning or even of the most ordinary education was taught geometry. Montucla touches on this subject, but not with his wonted accuracy, (I., 166.) Indeed, he seems to confound ancient with modern analysis, although no one has more accurately described and illustrated the ancient method, (I., 164, 275.) He adopts the erroneous notion of Plato having discovered this method, but he does not fall into the other error of ascribing to him the discovery of Conic Sections, (*ib.* 168.)

That he did not undervalue algebra and the calculus to which it has given rise, appears from many circumstances—among others, from what has already been stated; it appears also from this, that in many of his manuscripts there are found algebraical formulas for propositions which he had investigated geometrically. Maclaurin consulted him on the preparation of his admirable work, the 'Fluxions,' and received from him copious suggestions and assistance. Indeed, he adopted from him the celebrated demonstration of the fluxion (or differential) of a rectangle.* But Simson's whole mind, when left to its natural bent, was given to the beauties of the Greek geometry; and he had not been many months settled in his academical situation when he began to follow the advice which Halley had given him, as both calculated, he said, to promote his own reputation, and to confer a lasting benefit upon the science cultivated by them both with an equal devotion. It is even certain that the obscure and most difficult subject of Porisms very early occupied his thoughts, and was the field of his researches, though to the end of his life he never had made such progress in the investigation as satisfied himself. Before 1715, three years after he began his course of teaching, he was deeply engaged in this inquiry; but he only regarded it as one branch of the great and dark subject which Halley had recommended to his care. After he had completely examined, corrected, and published, with most important additions, the Conics of Apollonius, which happily remain entire, but which, as we have seen, had been most inelegantly and indeed algebraically given by De la Hire, L'Hôpital, and others, to restore the lost books was his great desire, and formed the grand achievement which he set before his eyes.

We have already shown how scanty the light was

* Book i., chap. ii., prop. 8.

by which his steps in this path must be guided. The introduction to the Seventh book of Pappus contained the whole that had reached our times to let us know the contents of the lost works. Some of the summaries which that valuable discourse contains are sufficiently explicit, as those of the Loci Plani and the Determinate Section. Accordingly, former geometricians had succeeded in restoring the Loci Plani, or those propositions which treat of loci to the circle and rectilinear figures. They had, indeed, proceeded in a very unsatisfactory manner. Schooten, a Dutch mathematician of great industry and no taste, had given purely algebraic solutions and demonstrations. Fermat, one of the greatest mathematicians of the seventeenth century, had proceeded more according to the geometrical rules of the ancients; but he had kept to general solutions, and neither he nor Schooten had given the different cases, according as the data in each proposition were varied; so that their works were nearly useless in the solution of problems, the great purpose of Apollonius, as of all the authors of the *τοπος αναλυομενου*—the thirty-three ancient books. As for the analysis, it was given by neither, unless, indeed, Schooten's algebra is to be so termed. Fermat's demonstrations were all synthetical. His treatise, though written as early as 1629, was only published among his collected works in 1670. Schooten's was published among his 'Exercitationes Mathematicæ' in 1657. Of the field thus left open, Dr. Simson took possession, and he most successfully cultivated every corner of it. Nothing is left without the most full discussion; all the cases of each proposition are thoroughly investigated. Many new truths of great importance are added to those which had been unfolded by the Greek philosopher. The whole is given with the perfect precision and the pure elegance of the ancient analysis; and the universal assent of the scientific world has even confessed that there is

every reason to consider the restored work as greatly superior to the lost original.

The history of this excellent treatise shows in a striking manner the cautious and modest nature of its author. He had completed it in 1738; but, unsatisfied with it, he kept it by him for eight years. He could not bring himself to think that he had given the "ipsissimæ propositiones of Apollonius in the very order and spirit of the original work." He was then persuaded to let the book appear, and it was published in 1746. His former scruples and alarms recurred; he stopped the publication; he bought up the copies that had been sold; he kept them three years longer by him; and it was only in 1749 that the work really appeared. Thus had a geometrician complied with the rule prescribed by Horace for those who have no standard by which to estimate with exactness the merit of their writings.

In the meantime he had extended his researches into other parts of the subject. Among the rest he had restored and greatly extended the work on Determinate Section, or the various propositions respecting the properties of the squares and rectangles of segments of lines passing through given points. There is no doubt that the prolixity, however elegant, with which the ancients treated this subject, is somewhat out of proportion to its importance; and as it is peculiarly adapted to the algebraical method, presenting, indeed, little difficulty to the analyst, the loss of the Pergæan treatise is the less to be deplored, and its restoration was the less to be desired. Apollonius had even thought it expedient to give a double set of solutions; one by straight lines, the other by semicircles. Dr. Simson's restoration is most full, certainly, and contains many and large additions of his own. It fills above three hundred quarto pages. His predecessors had been Snellius, whose attempt, published in 1608, was universally allowed to be a failure; and Anderson,

a professor of Aberdeen, whose work, in 1612, was much better, but confined to a small part only of the subject.

About the time that Dr. Simson finally published the Loci Plani, he began his great labour of giving a correct and full edition of the Elements. The manner in which this has been accomplished by him is well known. The utmost care was bestowed on the revision of the text; no pains were spared in collating editions; all commentaries were consulted; and the elegance and perfect method of the original has been so admirably preserved, that no rival has ever yet risen up to dispute with Simson's Euclid the possession of the schools. The time bestowed on this useful work was no less than nine years. It only was published in 1758. To the second edition, in 1762, he added a similarly correct edition of the Data, comprising several very valuable original propositions of his own, of Mr. Stewart, and of Lord Stanhope, together with two excellent problems to illustrate the use of the Data in solutions.

We thus find Dr. Simson employed in these various works which he successively gave to the world, elaborated with infinite care, and of which the fame and the use will remain as long as the mathematics are cultivated, some of them delighting students who pursue the science for the mere speculative love of contemplating abstract truths, and the gratification of following the rigorous proofs peculiar to that science; some for the instruction of men in the elements, which are to form the foundation of their practical applications of geometry. But all the while his mind never could be wholly weaned from the speculation which had in his earliest days riveted his attention by its curious and singular nature, and fired his youthful ambition by its difficulty, and its having vanquished all his predecessors in their efforts to master it. We have seen that as early as 1715 at the latest, probably much

earlier, the obscure subject of Porisms had engaged his thoughts; and soon after, his mind was so entirely absorbed by it that he could apply to no other investigation. The extreme imperfection of the text of Pappus, the dubious nature of his description, his rejection of the definition which appeared intelligible, his substituting nothing in its place except an account so general that it really conveyed no precise information, the hiatus in the account which he subjoins of Euclid's three books, so that even with the help of the lemmas related to these propositions of the lost work, no clear or steady light could be descried to guide the inquirer—for the first porism of the first book alone remained entire, the general porism being given wholly truncated (mancum et imperfectum)—all seemed to present obstacles wholly insurmountable, and after various attempts for years he was fain to conclude with Halley that the mystery belonged to the number of those which can never be penetrated. He lost his rest in the anxiety of this inquiry; sleep forsook his couch, his appetite was gone; his health was wholly shaken; he was compelled to give over the pursuit; he was "obliged," he says, "to resolve steadily that he never more should touch the subject, and as often as it came upon him he drove it away from his thoughts."*

It happened, however, about the month of April, 1722, that while walking on the banks of the Clyde with some friends, he had fallen behind the company; and musing alone, the rejected topic found access to his thoughts. After some time a sudden light broke in upon him; it seemed at length as if he could descry something of a path, slippery, tangled, interrupted, but still practicable, and leading at least in the direction towards the object of his research. He eagerly drew a figure on the stump of a neighbouring tree with a

* "Firmiter animum induxi hæc nunquam in posterum investigare. Unde quoties menti occurrebant, toties eas arcebam."—(Op. Rel. 320. Præf. ad Porismata.)

piece of chalk; he felt assured that he had now the means of solving the great problem; and although he afterwards tells us that he then had not a sufficiently clear notion of the subject (eo tempore Porismatum naturam non satis compertam habebam),* yet he accomplished enough to make him communicate a paper upon the discovery to the Royal Society, the first work he ever published (Phil. Trans. for 1723). He was wont in after life to show the spot on which the tree, long since decayed, had stood. If peradventure it had been preserved, the frequent lover of Greek geometry would have been seen making his pilgrimage to a spot consecrated by such touching recollections. The graphic pen of Montucla, which gave such interest to the story of the first observation of the transit of Venus by Horrox in Lancashire, and to the Torricellian experiment,† is alone wanting to clothe this passage in colours as vivid and as unfading.

This great geometrician continued at all the intervals of his other labours intently to investigate the subject on which he thus first threw a steady light.

His first care upon having made this discovery was to extend the particular propositions until he had obtained the general one. A note among his memoranda appears to have been made, as was his custom, of the date at which he succeeded in any of his investigations.‡—"Hodie hæc de porismatis inveni, R. S., 23 April 1722." Another note, 27th April, 1722, shows that he had then obtained the general proposition; he afterwards communicated this to Maclaurin when he

* Op. Rel. 320. † Hist. de Math. vol. i.

‡ In one there is this note upon the solution of a problem of tactions,—"Feb. 9, 1734:—Post horam primam ante meridiem;" and much later in life we find the same particularity in marking the time of discovery. His birthday was October 14, and having solved a problem on that day, 1764, he says—

	14 Octobr. 1764.
Deo Opt. Max. benignissimo Servatori	14 Octobr. 1687.
Laus et gloria.	77 (scil. Anno Ætatis.)

passed through Glasgow on his way to France; and on his return he communicated to Dr. Simson without demonstration a proposition concerning conics derived from it, which led his friend to insert some important investigations in his Conic Sections. In 1723 the publication of his paper took place in the Philosophical Transactions; it is extremely short, and does not appear to contain all that the author had communicated; for we find this sentence inserted before the last portion of the paper:—"His adjecit clarrissimus professor propositiones duas sequentes libri primi Porismatum Euclidis, a se quoque restitutas." The paper contains the first general proposition and its ten cases, and then the second with its cases. No general description or definition is given of Porisms; and it is plain that his mind was not then finally made up on this obscure subject, although he had obtained a clear view of it generally.

At what time his knowledge of the whole became matured we are not informed; but we know that his own nature was nice and difficult on the subject of his own works; that he never was satisfied with what he had accomplished; and he probably went on making constant additions and improvements to his work. Often urged to publish, he as constantly refused; indeed he would say that he had done nothing, or next to nothing, which was in a state to appear before the world; and moreover, he very early began to apprehend a decay of his faculties, from observing his recollection of recent things to fail, as is very usual with all men; for as early as 1751, we find him giving this as a reason for declining to undertake a work on Lord Stanhope's recommendation, when he was only in his sixty-fifth year. Thus, though he at first used to say he had nothing ready for publication, he afterwards added, that he was too old to complete his work satisfactorily. In his earlier days he used occasionally to affect a kind of odd mystery on the subject, and when

one of his pupils (Dr. Traill) submitted to him some propositions, which he regarded as porisms, Dr. Simson would neither admit nor deny that they were such, but said with some pleasantry, "They are propositions." One of them, however, he has given in his work as a porism, and with a complimentary reference to its ingenious and learned author.

Thus his life wore away without completing this great work, at least without putting it in such a condition as satisfied himself. It was left among his MSS., and by the judicious munificence of a noble geometrician, the liberal friend of scientific men, as well as the successful cultivator of science, Earl Stanhope,* it was, after his death, published, with his restoration of Apollonius' treatise De Sectione determinatâ, a short paper on Logarithms, and another on the Method of Limits geometrically demonstrated, the whole forming a very handsome quarto volume; of which the Porisms occupies nearly one-half, or 277 pages.

This work is certainly the master-piece of its distinguished author. The extreme difficulty of the subject was increased by the corruptions of the text that remains in the only passage of the Greek geometers which has reached us, the only few sentences in which any mention whatever is made of porisms. This passage is contained in the preface or introduction to the seventh book of Pappus, which we have already had occasion to cite. But this was by far the least of the difficulties which met the inquirer after the hidden treasure, the restorer of lost science, though Albert Girard thought or said, in 1635, that he had restored the Porisms of Euclid. As we have seen, no trace of his labours is left; and it seems extremely unlikely that he should have really performed such a feat and given no proofs of it. Halley, the most learned and able of Dr. Simson's predecessors, had tried the subject, and tried it

* Grandfather of the present Earl, whose father also was a successful cultivator of natural science, mechanical especially.

in vain. He thus records his failure:—"Hactenus Porismatum descriptio nec mihi intellecta nec lectori profutura." These are his words, in a preface to a translation which he published of Pappus's seventh book, much superior in execution to that of Commandini. But this eminent geometrician was much more honest than some, and much more safe and free from mistake than others who touched upon the subject that occupied all students of the ancient analysis. He was far from pretending, like Girardus, to have discovered that of which all were in quest. But neither did he blunder like Pemberton, whom we find, the very year of Simson's first publication, actually saying in his paper on the Rainbow—"For the greater brevity I shall deliver them (his propositions) in the form of porisms, as, in my opinion, the ancients called all propositions treated by analysis only" (Philosophical Transactions, 1723, p. 148); and, truth to say, his investigation is not very like ancient analysis either. The notion of D'Alembert, somewhat later, has been alluded to already; he imagined porism to be synonymous with lemma, misled by an equivocal use of the word in some passages of ancient authors, if indeed he had ever studied any of the writers on the Greek geometry, which, from what I have stated before, seems exceedingly doubtful. But the most extraordinary, and indeed inexcusable ignorance of the subject is to be seen in some who, long after Simson's paper had been published, were still in the dark; and though that paper did not fully explain the matter, it yet ought to have prevented such errors as these fell into. Thus Castillon, in 1761, showed that he conceived porisms to be merely the constructions of Euclid's Data. If this were so, there might have been some truth in his boast of having solved all the Porisms of Euclid; and he might have been able to perform his promise of soon publishing a restoration of those lost books.

It is remarkable enough that before Halley's attempts

and their failure, candidly acknowledged by himself, Fermat had made a far nearer approach to a solution of the difficulty than any other of Simson's predecessors. That great geometrician, after fully admitting the difficulty of the subject, and asserting* that, in modern times, porisms were known hardly even by name, announces somewhat too confidently, if not somewhat vaingloriously, that the light had at length dawned upon him,† and that he should soon give a full restoration of the whole three lost books of Euclid. Now the light had but broke in by a small chink, as a mere faint glimmering, and this restoration was quite impossible, inasmuch as there remained no account of what those books contained, excepting a very small portion obscurely mentioned in the preface of Pappus, and the lemmas given in the course of the seventh book, and given as subservient to the resolution of porismatic questions. Nevertheless Fermat gave a demonstration of five propositions, "in order," he says, "to show what a porism is, and to what purposes it is subservient." These propositions are, indeed, porisms, though their several enunciations are not given in the true porismatic form. Thus, in the most remarkable of them, the fifth, he gives the construction as part of the enunciation. So far, however, a considerable step was made; but when he comes to show in what manner he discovered the nature of his porisms, and how he defines them, it is plain that he is entirely misled by the erroneous definition justly censured in the passage of Pappus already referred to. He tells us that his propositions answer the definition; he adds that it reveals the whole nature of porisms; he says that by no other account but the one contained in the definition, could

* "Intentata ac velut disperata Porismatum Euclidæa doctrina.—Geometrici (ævi recentioris) nec vel de nomine cognoverunt, aut quod esset solummodo sunt suspicati."—(Var. Opera, p. 166.)

† "Nobis in tenebris dudum cæcutientibus, tandem se (Natura Porismatum) clara ad videndum obtulit, et purâ per noctem luce refulsit."—(Epist. ib.)

we ever have arrived at a knowledge of the hidden value;* and he shows how, in his fifth proposition, the porism flows from a locus, or rather he confounds porisms with loci, saying porisms generally are loci, and so he treats his own fifth proposition as a locus; and yet the locus to a circle which he states as that from which his proposition flows has no connexion with it, according to Dr. Simson's just remark ('Opera Reliqua,' p. 345). That the definition on which he relies is truly imperfect, appears from this: there could be no algebraical porism, were every porism connected with a local theorem. But an abundant variety of geometrical porisms can be referred to, which have no possible connexion with loci. Thus, it has never been denied that most of the Propositions in the Higher Geometry, which I investigated in 1797, were porisms, yet many of them were wholly unconnected with loci; as that affirming the possibility of describing an hyperbola which should cut in a given ratio all the areas of the parabolas lying between given straight lines.† Here the locus has nothing to do with the solution, as if the proposition were a kind of a local theorem: it is only the line dividing the curvilineal areas, and it divides innumerable such areas. Professor Playfair, who had thoroughly investigated the whole subject, never in considering this proposition doubted for a moment its being most strictly a porism.

Therefore, although Fermat must be allowed to have made a considerable step, he was unacquainted with the true nature of the porism; and instead of making good his boast that he could restore the lost books, he never even attempted to restore the investigation of the first proposition, the only one that remains entire. A better proof can hardly be given of the difficulty of the whole subject.‡

* Var. Op. p. 118.

† Phil. Trans. 1798, p 111.

‡ The respect due to the great name of Fermat, a venerable magistrate

Indeed it must be confessed that Pappus's account of it, our only source of knowledge, is exceedingly obscure, all but the panegyric which in a somewhat tantalizing manner, he pronounces upon it. "Collectio," says he, "curiosissima multarum rerum spectantium ad resolutionem difficiliorum et generaliorum problematum" (lib. vii. Proem). His definition already cited is, as he himself admits, very inaccurate; because the connexion with a locus is not necessary to the porismatic nature, although it will very often exist, inasmuch as each point in the curve having the same relation to certain lines, its description will, in most cases, furnish the solution of a problem, whence a porism may be deduced. Nor does Pappus, while admitting the inaccuracy of the definition, give us one of his own. Perhaps we may accurately enough define a porism to be the enunciation of the possibility of finding that case in which a determinate problem becomes indeterminate, and admits of an infinity of solutions, all of which are given by the statement of the case.

For it appears essential to the nature of a porism that it should have some connection with an indeterminate problem and its solution. I apprehend that the poristic case is always one in which the data become such that a transition is made from the determinate to the indeterminate, from the problem being capable of one or two solutions, to its being capable of an infinite number. Thus it would be no porism to affirm that an ellipse being given, two lines may be found at right angles to each other, cutting the curve, and being in a proportion to each other which may be

and most able geometrician, is not to be questioned. He was, indeed, one of the first mathematicians of the age in which he flourished, along with the Robervals, the Harriots, the Descartes. How near he approached the differential calculus is well known. His correspondence with Roberval, Gassendi, Pascal, and others, occupies ninety folio pages of his posthumous works, and contains many most ingenious, original, and profound observations on various branches of science.

found: the two lines are the perpendiculars at the centre, and are of course the two axes of the ellipse; and though this enunciation is in the outward form of a porism, the proposition is no more a porism than any ordinary problem; as that a circle being given a point may be found from whence all the lines drawn to the circumference are equal, which is merely the finding of the centre. But suppose there be given the problem to inflect two lines from two given points to the circumference of an ellipse, the sum of which lines shall be equal to a given line, the solution will give four lines, two on each side of the transverse axis. But in one case there will be innumerable lines which answer the conditions, namely, when the two points are in the axis, and so situated that the distance of each of them from the farthest extremity of the axis is equal to the given line, the points being the foci of the ellipse. It is, then, a porism to affirm that an ellipse being given, two points may be found such that if from them be inflected lines to any point whatever of the curve, their sum shall be equal to a straight line which may be found; and so of the Cassinian curve, in which the rectangle under the inflected lines is given. In like manner if it be sought in an ellipse to inflect from two given points in a given straight line, two lines to a point in the curve, so that the tangent to that point shall, with the two points and the ordinate, cut the given line in harmonical ratio; this, which is only capable of one solution in ordinary cases, becomes capable of an infinite number when the two points are in the axis, and when the ellipse cuts it; for in that case every tangent that can be drawn, and every ordinate, cut the given line harmonically with the curve itself.*

* The ellipse has this curious property, which I do not find noticed by Maclaurin in his Latin Treatise on Curve Lines appended to the Algebra, and dealing a good deal with Harmonical proportions. If from any point whatever out of the ellipse there be drawn a straight line in any

Dr. Simson's definition is such that it connects itself with an indeterminate case of some problem solved, but it is defective, in appearance rather than in reality, from seeming to confine itself to one class of porisms. This appearance arises from using the word "*given*" (*data* or *datum*) in two different senses, both as describing the hypothesis and as affirming the possibility of finding the construction so as to answer the conditions. This double use of the word, indeed, runs through the book, and though purely classical, is yet very inconvenient; for it would be much more distinct to make one class of things those which are assuredly data, and the other, things which may be found. Nevertheless, as his definition makes all the innumerable things not given have the same relation to those which are given, this should seem to be a limitation of the definition not necessary to the poristic nature. Pappus's definition, or rather that which he says the ancients gave, and which is not exposed to the objection taken by him to the modern one, is really no definition at all; it is only that a porism is something between a theorem and a problem, and in which, instead of anything being proposed to be done, or to be proved, something is proposed to be investigated. This is erroneous, and contrary to the rules of logic from its generality; it is, as the lawyers say,

direction whatever cutting the ellipse, the line is cut harmonically by the tangent, the ordinate, and the chords of the two arcs intercepted between the point of contact of the tangent and the axis. The tangent, sine, and chords are always an harmonical pencil, and consequently cut in the Harmonical ratio all lines drawn in all directions, from the given point. This applies to all ellipses upon the same axis, (all having the same subtangent,) and of course to the circle. The ellipse, therefore, might be called the *Harmonical Curve*, did not another of the 12th order rather merit that name, which has its axis divided harmonically by the tangent, the normal, the ordinate, and a given point in the axis. Its differential equation is $2\,d\,y^2 + d\,x^2 = \frac{y\,d\,y\,d\,x}{x}$, which is reducible, and its integral is an equation of the 12th order. There is also another Harmonical Curve, a transcendental one, in which chords vibrate isochronously.

void for uncertainty. The modern one objected to by Pappus is not uncertain; it is quite accurate as far as it goes; but it is too confined, and errs against the rules of logic by not being coextensive with the thing proposed to be defined.

The difficulty of the subject has been sufficiently shown from the extreme conciseness and the many omissions, the almost studied obscurity, of the only account of it which remains, and to this must certainly be added the corruption of the Greek text. The success which attended Dr. Simson's labours in restoring the lost work, as far as that was possible, and, at any rate, in giving a full elucidation of the nature of porisms, now, for the first time, disclosed to mathematicians, is, on account of those great difficulties by which his predecessors had been baffled, the more to be admired. But there is one thing yet more justly a matter of wonder, when we contrast his proceedings with theirs. The greater part of his life, a life exclusively devoted to mathematical study, had been passed in these researches. He had very early become possessed of the whole mystery, from other eyes so long concealed. He had obtained a number of the most curious solutions of problems connected with porisms and was constantly adding to his store of porisms and of lemmas subservient to their investigation. For many years before his death, his work had attained, certainly the form, if not the size, in which we now possess it. Yet he never could so far satisfy himself with what has abundantly satisfied every one else, as to make it public, and he left it unpublished among his papers when he died. Nothing can be more unlike those who freely boasted of having discovered the secret, and promised to restore the whole of Euclid's lost books. It is as certain that the secret was never revealed to them as it is that neither they nor any man could restore the books. But how speedily would the Castillons, the Alberts, even the Fermats, have

given their works to the world had they become possessed of such a treasure as Dr. Simson had found! Yet though ready for the press, and with its preface composed, and its title given in minute particularity, he never could think that he had so far elaborated and finished it as to warrant him in finally resolving on its publication.

There needs no panegyric of this most admirable performance. Its great merit is best estimated by the view which has been taken of the extraordinary difficulties overcome by it. The difficulty of some investigations—the singular beauty of the propositions, a beauty peculiar to the porism from the wonderfully general relations which it discloses—the simplicity of the combinations—the perfect elegance of the demonstrations—render this a treatise in which the lovers of geometrical science must ever find the purest delight.

Beside the general discussions in the preface, and in a long and valuable scholium after the sixth proposition, and an example of algebraical porisms, Dr. Simson has given in all ninety-one propositions. Of these four are problems, ten are loci, forty-three are theorems, and the remaining thirty-four are porisms, including four suggested by Matthew Stewart, and the five of Fermat improved and generalized; there are, besides, four lemmas and one porism suggested by Dr. Traill, when studying under the Professor. There may thus be said to be in all ninety-eight propositions. The four lemmas are propositions ancillary to the author's own investigations; for many of his theorems are the lemmas preserved by Pappus as ancillary to the porisms of Euclid.

In all these investigations the strictness of the Greek geometry is preserved almost to an excess; and there cannot well be given a more remarkable illustration of its extreme rigour than the very outset of this great work presents. The porism is, that a point may be found in any given circle through which all

the lines drawn cutting its circumference and meeting a given straight line shall have their segments within and without the circle in the same ratio. This, though a beautiful proposition, is one very easily demonstrated, and is, indeed, a corollary to some of those in the 'Elements.' But Dr. Simson prefixes a lemma: that the line drawn to the right angle of a triangle from the middle point of the hypotenuse, is equal to half that hypotenuse. Now this follows, if the segment containing the right angle be a semicircle, and it might be thought that this should be assumed only as a manifest corollary from the proposition, or as the plain converse of the proposition, that the angle in a semicircle is a right angle, but rather as identical with that proposition; for if we say the semicircle is a right-angled segment, we also say that the right-angled segment is a semicircle. But then it might be supposed that two semicircles could stand on one base: or, which is the same thing, that two perpendiculars could be drawn from one point to the same line; and as these propositions had not been in the elements, (though the one follows from the definition of the circle, and the other from the theorem that the three angles of a triangle are equal to two right angles), and as it might be supposed that two or more circles, like two or more ellipses, might be drawn on the same axis, therefore the lemma is demonstrated by a construction into which the centre does not enter. Again, in applying this lemma to the porism (the proportion of the segments given by similar triangles), a right angle is drawn at the point of the circumference, to which a line is drawn from the extremity of a perpendicular to the given line; and this, though it proves that perpendicular to pass through the centre, unless two semicircles could stand on the same diameter, is not held sufficient; but the analysis is continued by help of the lemma to show that the perpendicular to the given line passes through

the centre of the given circle, and that therefore the point is found. It is probable that the author began his work with a simple case and gave it a peculiarly rigorous investigation in order to explain, as he immediately after does clearly in the scholium already referred to, the nature of the porism, and to illustrate the erroneous definitions of later times (νεοτεριкοι) of which Pappus complains as illogical.

Of porisms, examples have been now given both in plain geometry, in solid, and in the higher: that is, in their connexion both with straight lines and circles, with conic sections, and with curves of the third and higher orders. Of an algebraical porism it is easy to give examples from problems becoming indeterminate; but these propositions may likewise arise from a change in the conditions of determinate problems. Thus, if we seek for a number, such that its multiple by the sum of two quantities shall be equal to its multiple by the difference of these quantities, together with twice its multiple by a third given quantity, we have the equation $(a+b)\ x=(a-b)\ x+2cx$ and $2bx=2cx$; in which it is evident, that if $c=b$, any number whatever will answer the conditions, and thus we have this porism: Two numbers being given a third may be found, such that the multiple of any number whatever by the sum of the given numbers, shall be equal to its multiple by their differences, together with half its multiple by the number to be found. That number is in the ratio of 4 : 3 to the lesser given number.

There are many porisms also in dynamics. One relates to the centre of gravity which is the porismatic case of a problem. The porism may be thus enunciated:—Any number of points being given, a point may be found such, that if any straight line whatever be drawn through it, the sum of the perpendiculars to it from the points on one side will be equal to the sum of the perpendiculars from the points on the other side. That point is consequently the centre

of gravity: for the system is in equilibrium by the proposition. Another is famous in the history of the mixed mathematics. Sir Isaac Newton, by a train of most profound and ingenious investigation, reduced the problem of finding a comet's place from three observations (a problem of such difficulty, that he says of it, "hocce problema longe difficilimum omnimodo aggressus,"*) to the drawing a straight line through four lines given by position, and which shall be cut by them in three segments having given ratios to each other. Now his solution of this problem, the corollary to the twenty-seventh lemma of the first book, has a porismatic case, that is, a case in which any line that can be drawn through the given lines will be cut by them in the same proportions, like the lines drawn through three harmonicals in the porism already given of the harmonical curve. To this Newton had not adverted, nor to the unfortunate circumstance that the case of comets is actually the case in which the problem thus becomes capable of an infinite number of solutions. The error was only discovered after 1739, when it was found that the comet of that year was thrown on the wrong side of the sun by the Newtonian method. This enormous discrepancy of the theory with observation, led to a full consideration of the subject, and to a discovery of the porismatic case.

When the studies of a philosopher, and especially of a mathematician, have been described, his discoveries recorded, and his writings considered, his history has been written. His private life is generally unvaried, filled with speculative inquiry, amused by scientific reading, variegated only by philosophic conversation, unless when its repose is broken by controversy, an incident scarcely possible in the story of mathematicians. Dr. Simson loved to amuse his leisure hours, and unbend his mind in the relaxation

* Principia, lib. iii., prop. xli.

of society; and from the simplicity of his manners and the kindliness of his disposition, as well as from his very universal information, he was ever a most welcome member of the circles which he frequented. He lived in his college chambers to the last, but received his friends occasionally at a neighbouring tavern, where a room was always kept at his disposal. He attended a club near the college, and in good weather its members dined every Saturday at Anderston, a suburb of Glasgow. In these meetings his chair was always reserved for him, being left vacant when he happened to be absent. It is also said to have been his habit to sit covered. He was fond of playing for an hour or two in the evening at whist, and of calculating chances, at which he generally failed; but he was on the whole a good player, though he was not very patient of his partner's blunders, nor always bore a bad hand of such partner with philosophic meekness. He was fond of music, and sometimes would sing a Greek ode to a modern air. Professor Robison says he twice heard him sing in this manner "a Latin hymn to the Divine Geometer," and adds, that the tears stood in his eyes as he gave it with devotional rapture. His voice was fine, says the Professor, and his ear most accurate. That he did not always interrupt his geometrical meditations in the hours of relaxation is very plain, not only from the singular anecdote already related of his discovery of porisms, but from the date of "Anderston" attached to some of his solutions, indicating that they had occurred to him while attending the Saturday meetings of the club in that suburb. In all his habits he was punctual and regular, even measuring the exercise which he took by the number of paces he walked. Anecdotes are related of him when interrupted by some one on his accustomed walk, and after hearing what was said, continuing at the number he had just before marked, and surprising his acquaintance by speaking the next

number aloud. He was exceedingly absent; and the younger part of the university pupils were wont to play upon this peculiarity. It is related that one of the college porters being dressed up for the purpose, came to ask charity, and in answer to the Professor's questions, gave an account of himself closely resembling his own history. When he found so great a resemblance, adds the story, he cried out, "What's your name?" and on the answer being given, "Robert Simson," he exclaimed with great animation, "Why, it must be myself!"—when he awoke from his trance. Notwithstanding his absent habits, he was an exceedingly good man of business; he filled the office of Clerk of the Faculty in the University for thirty years, and managed its financial and other concerns with great regularity and success. Like all minds of a higher order, his not only had no contempt for details, but a love of them; and while clerk he made a transcript with his own hand of the University records, for which he received a vote of thanks from the Senatus Academicus.

In 1758, being turned of threescore and ten, he found it necessary to employ an assistant; when Dr. James Williamson was appointed his helper and successor.* The University passed a resolution stating his merits fully, recording in detail his services to the college and to science at large, and pronouncing a warm but just panegyric upon him. He continued for ten years in the pursuit of his favourite studies, and the enjoyment of the same social intercourse as before. His health, which through his long life had been unbroken, remained entire till within a few weeks of its close, and he died on the first of October, 1768, having almost completed his eighty-first year.

He is represented to have been of a calm and pleasing presence, of a portly figure, of easy and not ungraceful manners. A portrait of him in the college library remains, and is said to do him justice. His

* This gentleman was not the favourite pupil Dr. Williamson who died at Lisbon.

pupil, Dr. Moore, the Greek professor, and author of the celebrated Grammar, also an excellent mathematician and great admirer of the ancient geometry, wrote the inscription which appears under it, marking its author's own taste in more ways than one:—

"Geometriam sub tyranno barbaro, sævâ servitute, diu languentem, vindicavit unus."

His character was lofty and pure: nothing could exceed his love of justice, and dislike of anything sordid or low; nor could he ever bear to hear men reviling one another, and, least of all, speaking evil of the absent or the dead. In this he closely resembled his celebrated pupil Mr. Watt. His religious as well as moral feelings were strong, and they were habitual. No one in his presence ever ventured on the least irreverent or indecorous allusion; and we find the periods of his geometrical discoveries mentioned with the date and the place, and generally an addition of "Deo" or "Christo laus," an example of which we have above presented.

He never was married. Of his brothers, one, Thomas, was Professor of Medicine at St. Andrew's, and author of an ingenious and original work on the Brain; his son succeeded him as professor. Another brother was a dissenting minister at Coventry; and a third, also settled there, had a son, Robert, first in the army, afterwards in the English Church—Mr. Pitt, probably from his love of the mathematics, having presented him to a living in the north of England. He was Dr. Simson's heir-at-law, and to him the estates were left. He sold them in 1789, as well Kirton Hill as Knock Ewart, which had been purchased by the Professor's father in 1713. A niece of Dr. Simson was married to Dr. Moore, the well-known novelist, and was mother of the General. That illustrious warrior was therefore great nephew of the mathematician. Mrs. Moore survived to a recent period, and died in extreme old age.

He bequeathed his mathematical library and manuscripts to the University of Glasgow, with special directions touching their disposition, custody, and use. They form, it is believed, the most complete collection of books and papers in that department of science anywhere to be seen.

The extraordinary genius of Dr. Simson for mathematical pursuits has been fully described in recording his achievements in that difficult branch of science. That he greatly furthered the progress of mathematical knowledge by his excellent publications of the elementary works of Euclid and Apollonius cannot be denied; nor can it be doubted that to him we owe a revival of the taste for the ancient analysis, the pure geometry, and the means now afforded of gratifying it. At the same time there is some room for lamenting that his great powers of mind and his patient industry of research were not devoted to the pursuit of more useful objects; and there is good reason to agree in the opinion expressed by one of his most eminent pupils, Professor Robison, that he might have better succeeded in his favourite object of recovering the purely geometrical methods of investigation, had he relaxed a little more from their rigour in applying them to the present state of science, and shown the ancient analytical investigation disencumbered of its prolixity, relieved from its extreme scrupulousness, and subservient to the investigations of the problems now become the main subjects of mathematical inquiry. This has in a great measure been performed by the most celebrated of his school, Matthew Stewart, who actually has solved Kepler's problem, and treated almost the whole doctrine of central forces by means of the ancient method.* At the same time we have only to cast our eye upon his diagrams to be convinced that

* His paper on the sun's distance, in which he also employs the ancient analysis, has been long since proved erroneous by my friend Mr. Dawson of Sedbergh, who wrote anonymously a demonstration of the error in 1772.

though he has solved the problems and demonstrated the theorems with a most wonderful skill, by means purely geometrical, yet he never could have obtained either the solutions or the demonstrations had not Newton preceded him, "his own analysis carrying the torch before."* The most celebrated proposition in all the 'Principia,' the general solution of the inverse problem of central forces,† (lib. i. prop. xli.) is closely followed by Stewart, and the diagrams are nearly the same.

This, however, is not the only ground of regret; for had it been so, the teacher's defect has been thus supplied by the scholar. But good cause remains to lament that both of those great masters did not abate somewhat of their devotion to the Greek Geometry, and instead of being captivated only with the view of its incomparable beauty, did not help forward by their discoveries those branches of the science which, though they may have far less grace, have yet a far wider range and far greater usefulness. Surely it is deeply to be lamented that such extraordinary powers of original investigation as both these great men possessed should, especially in the case of Stewart, have been wasted upon what Professor Robison's learned wit terms "a superstitious palæology," and in the overcoming of difficulties raised by themselves—of reaching the point in view by a devious and hard ascent, when a short and an easy path lay open before them—of doing, and not very well doing, by an imperfect though elegant tool, and with no help from machinery, the same work which might with far better success and greater facility have been performed by the most perfect instrument that ever man invented; as the laborious, patient, and ignorant Hindû, will with a knife carve

* "Suâ mathesi facem præferente."—*Halley.*

† I am aware of Professor Robison's statement, already cited, of Dr. Simson's opinion that the thirty-ninth proposition is the greatest of all, but I cannot help suspecting the forty-first to be intended.

some ivory trinket, on which he spends a lifetime that might have been employed in the most important works by the aid of fit implements—nay, who might have turned by a simple lathe myriads of the same kind of toy.

ADAM SMITH.

WITH AN ANALYSIS OF HIS GREAT WORK.

In the last years of the seventeenth century were born two men, who laid the foundation of ethical science as we now have it, greatly advanced and improved beyond the state in which the ancient moralists had left it, and as the modern inquirers took it up after the revival of letters, Bishop Butler and Dr. Hutchinson. The former, bred a Presbyterian, and exercised in the metaphysical subtleties of the Calvinistic school, had early turned his acute and capacious mind to the more difficult questions of morals, and having conformed to the Established Church, he delivered, as preacher at the Rolls Chapel, to which office he was promoted by Sir Joseph Jekyll, at the suggestion of Dr. Samuel Clarke, a series of discourses, in which the foundations of our moral sentiments and our social as well as prudential duties were examined with unrivalled sagacity. Dr. Hutchinson having published his speculations upon the moral sense, and the analogy of our ideas of beauty and virtue, while a young teacher among the Presbyterians in the north of Ireland, was afterwards for many years Professor of Moral Philosophy in the University of Glasgow, and there delivered his Lectures, which, by their copious illustrations, their amiable tone of feeling, their enlightened views of liberty and human improvement, and their persuasive eloquence, made a deeper impression than the more severe and dry compositions of Butler could ever create, and laid the foundation in Scotland of the modern ethical school.

In this he restored and revised, rather than created a taste for moral and intellectual science, which had prevailed in the fifteenth and early in the sixteenth centuries, but which the prevalence of religious zeal and of political faction had for above two hundred years extinguished. He restored it, too, in a new, a purer, and a more rational form, adopting, as Butler did nearly at the same time, though certainly without any communication, or even knowledge of each other's speculations, the sound and consistent doctrine which rejects as a paradox, and indeed a very vulgar fallacy, the doctrine that all the motives of human conduct are directly resolvable into a regard for self-interest.* Nothing more deserving of the character of a demonstration can be cited than the argument in a single sentence, by which he overthrows the position, that we seek other men's happiness, because by so doing we gratify our own feelings. This presupposes, says he, that there is a pleasure to ourselves in seeking their happiness, else the motive, by the supposition, wholly fails. Therefore there is a pleasure as independent of selfish gratification, as the thing pursued is necessarily something different from the being that pursues it.

These two great philosophers, then, may be reckoned the founders of the received and sound ethical system, to which Tucker, by his profound and original speculations, added much. Hartley and Bonnet, who were a few years later, only introduced a mixture of gross

* Hutchinson had taught his doctrines in Dublin some years before Butler's 'Sermons' were published in 1726, and had even published his 'Inquiry into Beauty and Virtue,' for the second edition of that work appeared in the same year. The 'Sermons' had indeed been preached at the Rolls, where he began to officiate as early as 1718; but nothing can be more unlikely than that any private intimation of their substance should have been conveyed to the young Presbyterian minister in Ireland. Indeed, his book was written soon after he settled at the academy, in 1716, which he taught near Dublin; for the Lord-Lieutenant, Lord Molesworth, who was appointed in that year, revised the manuscript of it. Butler and Hutchinson were contemporaries; one born 1692, the other 1694. Dr. Smith was born considerably later, in 1723; Mr. Hume in 1711.

error in their preposterous attempts to explain the inscrutable union of the soul and the body, and to account for the phenomena of mind by the nature or affection of the nerves; while at a somewhat earlier date, Berkeley, an inquirer of a much higher order, had applied himself to psychological, and not to ethical studies.

As ethics in its extended sense comprehends both the duties and capacities, and the moral and intellectual qualities of individuals, and their relations to each other in society, so may it also extend to the interests and the regulation of society, that is, to the polity of states in both its branches, both the structure and the functions of government, with a view to securing the happiness of the people. Hence it may include everything that concerns the rights, as well as the duties of citizens, all that regards their good government, all the branches of jurisprudence, all the principles that govern the production and distribution of wealth, the employment and protection of labour, the progress of population, the defence of the state, the education of its inhabitants; in a word, political science, including, as one of its main branches, political economy. When, therefore, ethical speculations had made so great progress, it was natural that this important subject should also engage the attention of scientific men; and we find, accordingly, that in the early part of the eighteenth century the attention of the learned and, in some but in a moderate degree, of statesmen also, was directed to these inquiries. Some able works had touched in the preceding century upon the subjects of money and trade. Sound and useful ideas upon these were to be found scattered through the writings of Mr. Locke. But at a much earlier period, Mr. Min, both in 1621 and 1664, had combatted successfully, as far as reasoning went, without any success in making converts, the old and mischievous, but natural fallacy, that the precious metals are the constituents of wealth. Soon after

Min's second work, 'The Increase of Foreign Trade,' Sir Wm. Petty still further illustrated the error of those who are afraid of an unfavourable balance of trade, and exposed the evil policy of regulating the rate of interest by law. A few years before Sir Wm. Petty's most celebrated work, his 'Anatomy of Ireland,' appeared Sir Josiah Child's 'Discourse of Trade,' 1668, in which, with some errors on the subject of interest, he laid down many sound views of trade, the principle of population, and the absurdity of laws against forestalling and regrating. In 1681 he published his 'Philopatris,' which shows the injurious effects of monopolies of every kind, and explains clearly the nature of money. But Sir Dudley North's 'Discourse,' published in 1691, took as clear and even as full a view of the true doctrines of commerce and exchange as any modern treatise; building its deductions upon the fundamental principle which lies at the root of all these doctrines, that, as to trade, the whole world is one country, of which the natives of each state severally are citizens or subjects; that no laws can regulate prices; and that whatever injures any one member of the great community injures the whole.

It must be observed that beside the treatises thus early published on œconomical science, we find occasionally very sound doctrines unfolded, and very just maxims of policy laid down, by well known writers, who incidentally touch upon œconomical subjects in works written with other views. Thus Fenelon, in his celebrated romance of 'Telemachus,' has scattered various reflexions of the truest and purest philosophy, upon the theory of commercial legislation, as well as upon many other departments of administration. It is due to the memory of a Romish prelate, and a royal preceptor in an absolute monarchy, to add that all his writings breathe a spirit of genuine religious tolerance, and of just regard to the civil rights and liberties of mankind.

In the eighteenth century, the writers of Italy appear to have taken the lead in these inquiries. The active and lively genius of the people, the division of the country into small states, the access to the ears of the Government which this naturally gives to learned men, the interest in the improvement of his country which the citizen of a narrow community is apt to feel, gave rise to such a multitude of writers on subjects of political economy, that when the Government of the Italian Republic, with a princely liberality, directed Custodi to publish a collection of their works at the public expense, in 1803, they were found to fill no less than fifty octavo volumes.

The earliest of these writings, which lay down sound principles to guide commercial legislation, is the Memoir ('Discorso Economico') of Antonio Bandini of Siena, addressed in 1737 to the Grand Duke of Tuscany upon the improvement of the great Maremma district. The author recommended free trade in corn; advised the granting of leases to tenants, that they might have an interest in the soil; and proposed the repeal of all vexatious imposts, and a substitution in their stead of one equal tax upon all real property, without excepting either the lands of the nobles or of the church. This able and enlightened work, in which the germs of the French economical doctrines are plainly unfolded, was only published in 1775; but when Leopold succeeded his brother in 1765, he showed his accustomed wisdom and virtue in the government of Tuscany, by adopting many of Bandini's suggestions for improving the Maremma. Other writers followed in the same course. Fernando Galiani, of Naples, published in 1750 his treatise, 'Della Moneta,' explaining on sound principles that the precious metals are only to be regarded as merchandise, and showing clearly the connexion between value and labour. The discourse, *Sopra i Bilanci delle Nazione*, by Carli, of Capo d'Istria, in 1771, laid down the true doctrine respecting the balance of trade.

Genovesi, a Neapolitan, in 1768, supported the position of perfect freedom in the corn trade, though not in that of other merchandise or of manufactures. But in 1769, Pillo Verri, a Milanese, in his work, '*Sulle Leggi Vincolanti*,' maintained the doctrine of absolute and universal freedom of commerce. The same thing was maintained about the same period in the work of Ferdinando Paoletti, a Florentine, entitled, '*Veri mezzi di rendere felice le Società*.' So that, both before and after the French economists began their useful and enlightened labours, the fundamental doctrine of Adam Smith's celebrated work had been laid down by a great number of writers in the different parts of the Italian Peninsula.*

The progress made in France by the same class of philosophers and statesmen was very considerable, and about the same time. Although the Italian writers rather preceded, yet there is no doubt their works were unknown beyond the Alps for many years after the French had applied themselves successfully to the cultivation of economical science. It is supposed, and apparently with reason, that a mercantile man, who also held the rank of a landed gentleman, Vincent Seigneur de Gournay of St. Malo, educated for trade at Cadiz, but always a bold thinker and a diligent student, was the first who adopted the principles of a liberal and enlightened commercial policy. His reputation both as an eminent merchant and as a learned inquirer had become considerable, when he was appointed, in 1751, to the office of *Intendant de Commerce*, answering in some sort to our President of the Board of Trade. His administration was a constant struggle with the narrow prejudices of the old system, which rests on encouragement, protection, prohibition, end-

* Not having access to Custodi's work, and only having seen some of the treatises contained in it, I have relied on the statement given in the learned article on Political Economy, ('Penny Cyclopædia,' vol. xviii., p. 339-40.).

less intermeddling with the distribution of capital, and the employment of labour. He was so often and so powerfully thwarted, that his reforms were anything but complete. All he attempted was in the right direction; and M. Turgot, his disciple, who afterwards, in his own administration of the higher department of finance, carried the same views farther, has given us a luminous abstract of those sound principles which De Gournay laid down. The duty of government, according to him, was to give all branches of industry that freedom of which the monopolizing spirit of different classes had so long deprived them; to protect men in making whatever use they please of their capital, their skill, their industry; to open among the makers and sellers of all goods the greatest competition, for the benefit of the buyers in the low price and good quality of the things sold, and among buyers the greatest competition, that the producer or the importer may have the due stimulus to his exertions; and to trust the natural operations of men's interests for the increase of national wealth and the general improvement of society, when all fetters are removed, and all absurd and pernicious encouragements by the State withheld.

It was not for some years after these enlightened and rational principles had been adopted, promulgated, and acted upon by M. de Gournay, that Dr. Quesnay, who had, from his youth upwards, attended to agricultural questions, and even somewhat to farming pursuits, but had been always immersed in the studies of his profession, began to cultivate economical science. He had published several works of the greatest ability and learning on medical and surgical subjects, had acquired extensive practice, and risen to the rank of the King's first physician* before he had

* A very interesting work was published by my worthy friend Mr. Quintin Crawford, in his 'Mélanges d'Histoire et de Littérature,' being the journal of Madame de Hausset, the waiting gentlewoman of Madame

matured his speculations so as to publish any treatise on political subjects; and though he was eighteen years older than M. de Gournay, the latter had been several years at the head of the commercial administration before the Doctor's first work appeared—his excellent papers on the Corn Trade in the Encyclopædia.* His celebrated '*Tableau Economique*,' in which the accumulation and distribution of wealth is stated with great ingenuity and originality, though in a somewhat abstruse form, appeared in 1758; and his greatest work, the '*Physiocratie*,' ten years later. His doctrine was, that the cultivation of the soil alone adds to the wealth of any state; that they alone who till the ground are entitled to be called productive labourers; that their industry alone yields a net or

de Pompadour. It contains some anecdotes of Dr. Quesnay extremely curious and characteristic, and shows on what an intimately familiar footing the great philosopher lived with the royal voluptuary, who had the sense to relish his conversation, and used to call him "his thinker," (mon penseur.) Mr. Crawford gives an accurate sketch of his character; and after mentioning that his followers always termed him "Le Maître," and decided their disputes by "Le Maître l'a dit," like the disciples of Plato, he tells us that, at his death, a funeral oration was pronounced by M. de Mirabeau, before the assembled sect, all in deep mourning. He adds, what may easily be believed, that this discourse was a "chef-d'œuvre de ridicule et d'absurdité." A great discussion, as it seems to me on a question very unimportant, has been raised by political economists, not much to the credit of their philosophical feelings, whether Quesnay's family were of as low a station as some represent them, and whether it be really true that they could not afford to have him taught to read in his boyhood. Surely the Memoirs of the Academy must be reckoned a decisive authority on this question. In the historical part of the volume for 1774, it is distinctly stated, as a matter well known, (p. 122,) that his father was an Avocat au Parlement de Montfort, and an intimate friend of the Procureur du Roi. Grimm mentions Quesnay in a very different manner from most others. He thus speaks of the economists and the great founder of their sect:—"Depuis que l'œconomie politique est devenue en France la science à la mode, il est formé une secte qui a voulu dominer dans cette partie. M. Quesnay s'est fait chef de cette secte."—"Le vieux Quesnay est un cynique décidé. M. de Fobernais n'est pas tendre; ainsi cette querelle ne se passera pas sans quelques faits d'armes." (*Corr.*) He repeatedly gives him the same epithet of *cynique;* probably the light conversation of Grimm had not attracted his notice, or gained his respect.

* The article 'Fermier' appeared in 1756; 'Grains' in 1757; M. Turgot's able articles appeared in 1756.

clear produce ('produit net') in the shape of rent over and above the expense of raising it by paying the workman's wages, and replacing with the ordinary profit the capital expended; that all other labour, as that of manufacturers who fashion the raw produce, of merchants or retail dealers who distribute it, whether raw or worked up, and professional men who do not operate upon produce at all, are, though highly useful, yet wholly and all equally unproductive, because those classes only receive their wages, or the profit of their stock, from the productive class—the agriculturists. From this theory he deduced practical inferences all of great importance, but of different degrees of value or accuracy; that all commerce, both external and internal, both in the raw and manufactured produce of any country, should be left entirely free; that all industry of every class should be alike unfettered; that all men should be left to employ their capital and their labour as their own view of their own interest directs them; that no tax should be imposed on any goods or any labour except a single impost, and that upon the net produce, the rent of land—this (the impôt foncière) taking the place of all others, and alone being levied to support the state.

Dr. Quesnay's ingenuity and learning, the boldness of his views, their great simplicity, their originality, all made a powerful impression; but from these very causes, and still more from the harshness and obscurity of the style in which they were unfolded—perhaps one might say enfolded,—they were better calculated to find acceptance with the learned few than with the general mass of readers. Upon these few, however, they soon made a deep impression, which was increased by their author's simple and amiable manners, his exemplary purity, though living in a corrupt court, and the admirable talent which he had in conversation, of exposing his doctrines, like our Franklin, by the aid of apposite fables or apologues. He became thus

easily the leader, or head of a sect, and he was looked up to by his disciples with the same reverence that the followers of the ancient sages paid to the objects of their veneration. The Marquis of Mirabeau, father of the famous revolutionary leader; M. Mercier de la Rivière; M. Dupont de Nemours; M. Condorcet, and M. Turgot, for some time Controller General of the Finances, were the most celebrated of this school. Their chief died as early as 1774, but they continued to instruct mankind by their writings, which, however ingenious and learned, were almost all deprived of their full effect upon the bulk of readers, by the dry, scholastic, and even crabbed style in which they were composed, and the want of that simple arrangement and that plain manner of unfolding their system which forms the first and the essential merit of didactic composition.

It must be added that on the structure of government, the doctrines of the sect were far less enlightened than upon its functions. While they held the whole happiness of society to depend upon a wise and honest administration of the supreme power in the state, they never considered how necessary it was to provide a security for that course being pursued, by establishing checks upon the rulers. Their doctrine was that what they called a *despotisme légale*, or an absolute power vested in the sovereign, and exercised according to fixed laws, is the most perfect form of government; and they entirely forgot that either no change whatever can be made in these laws, let ever so great a change happen in the circumstances of the community, or that all laws may be abrogated or altered at the monarch's pleasure, and thus, that the epithet "legal" dropt from their definition. In short they forgot, that their theory to be tolerable required the despot to be an angel, in which case, no doubt, their constitution would be perfect, but in no other. It is singular, that with all this, we find in the authen-

tic accounts of their founder's habits that he never could feel at his ease in the presence of Louis XV., and confessed the reason to be, his thinking all the while that he stood before a man who had the power of destroying him. This is recorded in the Memoirs to which I have above referred, and we find two instances in the same work, illustrating the practical operation of the "*despotisme légale.*" To the Doctor's great dismay, M. de Mirabeau, his steady follower, was suddenly hurried away to the fortress of Vincennes, because an expression in his speculative work on Taxation being misunderstood by the King, had given him offence; and when Turgot was anxious to obtain the King's assent, on the occasion of his proposing one of the great municipal reforms which he supported, he took the indirect, if not humiliating course of speaking to the Doctor and to the mistress's waiting-woman, to whom the Doctor gave a note of the plan, which by this circuit reached the Royal ear.

But our view of what has been accomplished in economical science, before the period to which the following Life refers, would be most imperfect, if we passed over the Essays of Mr. Hume. They were published in 1752, and gave the first clear refutation of the errors which had so long prevailed in Commercial Policy, and the first philosophical as well as practical exposition of those sound principles, which ought to be the guide of statesmen in their arrangements, as well as of philosophers in their speculations upon this important subject. I have treated of this admirable work in the life of that illustrious writer.*

It was necessary to give a summary of the progress which had been made in ethical and economical philosophy before the time of Dr. Smith, in order that we might duly appreciate the invaluable services which he rendered to both those branches of science, and to

* Vol. ii.

prevent us from supposing, as men are always prone to do, that he whose merit as a great improver can hardly be estimated too highly, was also the creator of the system which he so largely contributed to extend and to consolidate. We may now proceed to the history of his life.

Adam Smith was born at Kirkaldy, in the Scotch county of Fife, on the 5th of June, 1723, and was a posthumous child, his father having died a few months before. That gentleman was Controller of the Customs at the port, having been originally bred to the law, and afterwards held the office of Private Secretary to Lord Loudon, Secretary of State, and keeper of the Great Seal. His wife was a daughter of Mr. Douglas, of Strathenry. They had no other child but the philosopher, whose education devolved upon his mother, and was most carefully and affectionately conducted.

When a child of only three years old, he was stolen by a gang of the vagrants, called in Scotland, tinkers, and resembling gipsies in their habits—the same race which Fletcher of Saltoun describes as having in his day become so numerous that they formed a considerable proportion of the Scottish people. It was a fortunate circumstance, that being soon missed, his uncle, at whose house he was residing, pursued the wretches, and restored him to his affrighted parent.

He received the first rudiments of his education at the school of David Miller, an eminent teacher, several of whose pupils filled important public stations in after life. Being of weak constitution in his early years, books formed his only amusement, and his companions retained all their lives a lively recollection of his devotion to reading and of the great tenacity of his memory. He was also remarkable even in those early days for that absence which so distinguished him in company ever after. At the age of fourteen, as is usual in Scotland, he was sent to the University, and

remained at Glasgow for three years, when he obtained an exhibition to Baliol College.

At Oxford he remained for seven years, and applied himself to the acquisition of various learning. He became master of both ancient and modern languages, and exercised himself in translation, especially from the French, a mode which, like his illustrious friend Robertson, he always recommended, as tending to improve the student's style, by giving a facility in the use of his own language. But it is somewhat remarkable that his chief study was of mathematical and physical science, a walk little frequented at the University, and which, except as subservient to other speculations, he himself appears to have ever after abandoned. For some time, however, he must have retained both the taste and the capacity for those exalted studies; for Mr. Stewart recollects his father, the celebrated geometrician, reminding him of a problem proposed to him by Dr. Smith, which had occupied his attention after he had left College, and had come to reside at Glasgow.

On his return from Oxford he went to reside for two years at Kirkaldy, with his mother, for whom he entertained through his whole life an extraordinary and a perfectly well-grounded affection, being ever happier in her society than in any other; and he enjoyed the unspeakable blessing of having her days prolonged till he had himself reached a good old age. The plan of his family had been, that he should enter the English Church, and with this view he had been sent to Oxford. But Mr. Stewart says, that he did not find this profession suit his tastes; perhaps it did not accord with his habits of thinking; certain it is that he abandoned all thoughts of it, and contented himself with those chances of very moderate preferment which the Scotch Universities present to lovers of literature and science.

It is clearly proved by the course and by the tone of

his remarks on English universities,* that the discipline and habits of Oxford had in no way gained either his affection or his respect. Probably he could not easily forget the silly bigotry which caused his superiors to seize his copy of Hume's 'Treatise of Human Nature' when he was surprised reading it, and to administer a reprimand for the offence.

In 1748 he removed to Edinburgh, accompanied by his mother; and he read for about three years a course of Lectures on Rhetoric under the patronage of Lord Kames, himself a very succcessful follower of critical studies, and whose writings were the first to introduce in this island a sound philosophy upon those subjects. Dr. Smith also became intimately acquainted with the eminent men of letters who then adorned the Scottish capital, and some of whom were not yet well known to the world. Mr. Hume, Dr. Robertson, Dr. Blair, were among those literary men; Mr. Wedderburne afterwards Lord Loughborough, and Mr. Johnstone afterwards Sir William Pulteney, were severally members of the Scottish Bar. In 1751 he was elected to the Professorship of Logic in the University of Glasgow, which he exchanged the year after for that of Moral Philosophy. It had till four years before been filled by Hutcheson, under whom he had studied with all the admiration which the ingenuity and eloquence of that great teacher so naturally inspired, and with the affection which was commanded by his amiable character.

This important situation of a public teacher, one of the most exalted to which any man can aspire, was certainly of all others the most perfectly adapted to his genius, as it was the best suited to his habits and his tastes; for the love of speculation was in him combined with the desire of communicating information to others and of promoting their improvement. Even in society all his life, there was something didactic in the style of

* 'Wealth of Nations,' book v., chap. 1

his conversation. He was fond of laying down principles, illustrating them, and tracing their consequences. He was not, indeed, in such careless discussions, always either very practical or very reflecting and circumspect as to conclusions; and his hasty opinions, whether of men or of things, were often the result of momentary impressions, which he was quite ready to correct upon reconsideration. But the interest which he took in his subject always animated his discourse; and no one could more appropriately, or with greater claims to his hearer's attention, illustrate the bearing of the truths which he meant to convey. His language, too, was choice, both elegant, various, and plain; his manner having been formed upon the best models which he had, as we have seen, diligently studied, as indeed he had the principles of rhetoric, the subject of his earliest lectures. Nor had he any difficulty of extempore composition, though like many greater speakers, he at first was apt to hesitate until he became warmed with his subject, and then he could prelect with as great fluency of language as copiousness of illustration. It may thus be well supposed that on the subjects of his lectures, when he had given them the full consideration which was required for preparing himself, he could convey instruction in a manner at once sound, luminous, and attractive. Accordingly we find all accounts agree in representing him as a teacher of the very highest order, and his pupils as receiving instruction with a respect approaching to enthusiasm. Even the talents of Hutcheson had failed to recommend these studies to as general and cordial acceptation. The taste for metaphysical and ethical inquiries was greatly increased; discussions of the doctrines he taught became the favourite occupation in all the literary circles, and formed the subjects of debate in the clubs and societies of the place; even the peculiarities of his manner and pronunciation were eagerly caught up and imitated, though there was nothing which he less affected than the graces of delivery,

and nothing in which he less excelled; but it seemed like the free and spontaneous tribute to genius and learning which courtly servility had paid to one monarch by assuming his wry neck, and to another by adopting his false grammar,* so that he may perhaps be allowed to have more than any other celebrated teacher of our own times, attained the observance with which the ancient sects cultivated their masters, while his friend and coadjutor, De Quesnay, in this respect passed all who never actually taught.

The late eminent Professor Millar, who had been a pupil of Dr. Smith's, and who remained to his death one of his most intimate friends, has given a valuable account of his lectures which Mr. Stewart inserted in his 'Biographical Sketch.' When he taught the Logic Class, he appears to have rather converted the course into one upon rhetoric and belles lettres, only giving an introductory view of the School Logic and Metaphysics. The reason given for what appears to me a great departure from the proper duties of that chair, is, that he considered the best illustration of the mental powers to consist in examining the several ways of communicating our thoughts by speech, and tracing the principles upon which literary composition becomes most subservient to persuasion or entertainment. It really seems difficult to imagine a more unsatisfactory reason for teaching rhetoric as logic. The difference of the two studies was much more accurately perceived by another great light,—Lord Coke, who places them rather in contrast than in resemblance to each other, when he quaintly compares the original writ to logic, and the count or pleading to rhetoric, which assuredly it only resembles in being as unlike logic, as the plea is unlike the writ. But I apprehend, that whatever might be given as a *ratio justifica*, the *ratio suasoria* was

* Augustus and Louis XIV. Happily the Roman parasites could not, like the Parisians, bequeath their monarch's deformity, but *mon carosse* is still French.

the accidental possession of a course of lectures already delivered at Edinburgh in his earlier years; and that, had this course been directed to explain the learning of the Schools, the rules of argumentation, the principles of classification, and the limits of the various branches of science, the proper office of logic, we should not have heard of the somewhat unaccountable theory which has been cited from Mr. Millar's note.

After one course, however, of this description, he taught Moral Philosophy for twelve years, with extraordinary ability and the greatest success. It is most deeply to be lamented that of the four branches into which his course was divided, the two most interesting should not have reached us, the MS. having been destroyed a short time before his death. He first unfolded the sublime and important truths of Natural Theology, and the faculties and principles of the mind on which it rests, by far the most elevated of all human speculations, and one, as Archbishop Tillotson* has most soundly declared, which so far from being worthy of jealousy on their part who maintain the doctrines of Revelation, is of necessity the very foundation essential to support its fabric. Whether we regard the hopes of man as built upon his unassisted reason, or as confirmed by the light of religion, no study can match

* "All religion is founded upon right notions of God and his perfection, insomuch that divine revelation itself does suppose those for its foundations, and can signify (disclose or reveal) nothing to us unless they be first known and believed. For unless we be first firmly persuaded of the providence of God and of his superintendence over mankind, why should we suppose that he makes any revelation of his will to us? Unless it be first actually known that God is a God of truth, what ground is there for believing his word? So that the principles of natural religion are the foundations of that which is revealed." (Serm. xli.) This sermon was preached before the King and Queen, 27th October, 1692, at the thanksgiving for the naval victory, and contains even a more searching exposure of the errors of Romanism than the celebrated sermon (xl.) on the Church of Rome. The sermon on "Steadfastness in Religion," seems to me his Grace's other masterpiece in contending with Rome. It is a demonstration of the great practical doctrine of the right of private judgment, and it tallies in spirit with the above passage in the 41st.

that of Natural Theology in the loftiness of its nature, and the importance of its tendency.—"Neque enim homines ad Deos ulla re propius accedunt quam salutem hominibus dando." (Cic. 'Pro Lig.') He next explained the doctrines of Ethics, or the rules and principles by which men judge of the qualities in point of wisdom and goodness, of human action. The third division of his course was, properly speaking, a branch of the second; it embodied general jurisprudence, the structure of government, and the theory of legislation. In the fourth and last branch he treated of the principles upon which the wealth, power, and generally the prosperity of communities depend, and of the institutions relating to commerce, finance, instruction, and defined, in a word, the functions of government as contradistinguished from its structure. Of the second and fourth divisions he afterwards gave the substance in his published works; unhappily, the whole of his papers containing the first and the third series of Lectures, were destroyed by himself some time before he died, together with the Lectures on Rhetoric, which are described by Mr. Millar as having been composed with extraordinary care, and as having contained critical discussions of great delicacy of taste, as well as extensive learning. I cannot help regarding it as a circumstance, however unfortunate for the world, peculiarly happy for his executors, that these invaluable manuscripts were not left in their hands, with the injunction which his will contained to burn them, for if ever men can be conceived to lie under a temptation to strain at placing their public duty in opposition to their private obligations, it certainly would have been those eminent persons, Dr. Black and Dr. Hutton, shrinking from the painful office of performing the trusts of their friend's will.

While Dr. Smith was engaged in the duties of his Professorship at Glasgow, he published the first works which he gave to the world. In 1755 he contributed

to the 'Edinburgh Review,' of which I have spoken in the 'Life of Robertson,' a paper of great merit, being a criticism on Johnson's Dictionary. Allowing full praise to the merits of that important work, he yet very clearly showed the want of strict philosophical principle with which it is justly chargeable, the different senses of words being rarely arranged in classes, or the particular modifications of each signification under the more general, and as it were leading or prevailing sense, and words apparently synonymous, being very often distinguished with little care. He illustrates his remarks by examining the words, *but* and *humour*, as given by Johnson, and by giving them on his own more systematic plan. The article is masterly in all respects, and carries conviction to every attentive reader. The specimen is as well executed as possible, and makes it a matter of regret, not indeed that the author should have confined his own labours as a lexicographer, to pointing out the way instead of walking in it himself, but that his plan should not have been adopted and executed by others whose labour might have been better spared for so useful a work. This service to letters, indeed to science itself, still remains to be rendered, and if individuals should be scared from so toilsome an undertaking, it seems well suited to the joint exertions of some literary society. The zeal and activity of Voltaire, it may be mentioned, broke out almost on his death-bed, in persuading his colleagues of the Academy to accomplish a work of this kind, in some sort fellow to the one I speak of; for it was to remodel their Dictionary, giving the historical progress of the meaning attached to the words, with quotations from contemporary writers, and each Academician was to have taken a letter; he had begun himself to write upon the letter A, with his wonted industry, when that hand arrested him, to which the laborious and the idle alike must submit, closing his long and brilliant career.

Dr. Smith's other paper in the Review is a letter to

the editors upon the propriety of extending their plan, which had been confined to the criticism of works published in Scotland. He enters at some length into the general state of literature on the Continent, and shows a familiar acquaintance with it, that could only have been acquired by very extensive reading in the works of foreign writers. The advice which he gave would in all probability have been followed; but the Review was given up, as I have elsewhere stated,* in consequence of the ferment excited by the fanatical part of the Kirk.

In 1759 Dr. Smith published his 'Theory of Moral Sentiments,' being the greater part of the second division of his course of lectures, and the explanation of the principles upon which his ethical system rested. To the 'Theory' was subjoined a 'Dissertation on the Origin of Language,' a subject to which he had paid great attention. There is some doubt whether this was not added to the second edition of the work. Mr. Stewart is inclined to think that it was not in the first, but a different opinion has been confidently expressed by others. The success of this publication was great, and it was immediate. The book became at once generally popular; and Mr. Hume, who was in London at the time of its first appearance, wrote him a most lively and humorous letter, in which he gives the history of his friend's complete success. In this letter there is mentioned a circumstance, too, which we shall presently see was destined to have a great influence on his future prospects. The celebrated Charles Townsend said, on reading the book, that he should make it worth the author's while to undertake the charge of the young Duke of Buccleugh's education, whose mother, the dowager Duchess, he had married.

The success of this excellent work, however, was confined, at least for a long time, to the author's own

* 'Life of Robertson,' vol. ii.

country. It was soon translated into French, and the publisher sought to give it more attraction by adding an absurd title to the original one—he called it 'Métaphysique de l'Ame.' Grimm commends this as extremely clever; but adds that it had failed to obtain for the book any attention, and that it had entirely failed at Paris, which, however, he observes, proved nothing against its merits.*

After the 'Theory of Moral Sentiments' was published, Dr. Smith naturally made considerable changes in his course of lectures during the four years that he remained in Glasgow College. He greatly curtailed the second branch, having incorporated so large a portion of it in his book; and he extended the third and fourth heads—those parts which related to jurisprudence and political economy—giving more copious illustrations of the principles on which these important sciences are grounded. In particular, his discussions of commercial policy were more elaborately conducted; and he profited by his intimacy with merchants and manufacturers of eminence in the great trading city in which he resided to obtain practical information which might illustrate, if not guide, his speculative views—possibly also correcting those views by bringing them to the test of experience by free discussion.

The progress of his opinions in making converts to the modern doctrines concerning trade is represented as having been considerable, even among those whose prejudices in favour of the older maxims were of long standing; but of course his philosophy was more readily adopted, and more extensively diffused by the pupils, who came to the consideration of the subject with no

* "On a traduit depuis quelque tems la 'Théorie des Sentimens Moraux,' de M. Adam Smith, Professeur à Glasgow, en deux volumes in 8vo. Le traducteur ou le libraire, pour lui donner un titre plus piquant, l'a nommé spirituellement 'Métaphysique de l'Ame;' cet ouvrage a beaucoup de réputation en Angleterre, et n'a eu aucun succès à Paris. Cela ne décide rien contre son mérite." (Corr. IV., 291.)

bias upon their minds from former habits of thinking or long-formed professional opinions.

In 1763 the project, already mentioned, of Charles Townsend, was carried into execution, and Dr. Smith was induced to resign his professorship, with the view of attending the Duke of Buccleugh on his travels. The settlement of an adequate annuity upon him made this arrangement one sufficiently consistent with ordinary prudence. But it may reasonably be doubted, if, after enjoying the advantages of a residence during a year or two abroad, his happiness would not have been better consulted by returning to the duties and habits of his academical life. Nothing, certainly, can be more clear, than that the official appointment to which this change in his plans ultimately led, was one deeply to be lamented, and indeed to be disapproved in every respect, however well meant. It is somewhat humbling to our national pride to reflect that our Government could find no better employment, and no fitter reward for the most eminent philosopher of the age, than making him a revenue officer. For the last twelve years of his precious life he was condemned to go through the routine business of a Commissioner of the Customs;—as, some time after, one of the greatest poets who ever appeared in this island was made an exciseman, at seventy pounds a-year, for a bare subsistence, and daily threatened with removal, to die of hunger, if he did not square his conversation by the opinions on French politics which his superiors entertained.*

It must, however, be added, that nothing could better suit Dr. Smith, than the opportunity which his connexion with the Duke gave him of visiting France

* It is a gratifying proof of the progress which has since those times been made, that no minister could in our day propose such preferment to such men. An instance may probably be cited of an eminent poet being early in this century so employed; but there was a wide difference in the emoluments, and the place was nearly a sinecure.

and Switzerland. They repaired in the spring of 1764 to Paris, where they only remained a few days, and proceeding to Toulouse, passed in that provincial capital a year and a-half. Except that the French spoken on the Garonne is by no means so pure as that of the Loire and other districts in the centre of the country, the place was well chosen for a residence connected with education. There was an university of good repute with an excellent library; it was also the seat of one of the most important parliaments, and of an engineer and artillery head-quarters (a requisite of good society in my late friend Mr. Wickham's opinion); the society was polished and not dissipated, commerce and manufactures having somewhat unaccountably never established themselves in a city which seems well-suited to both from its central position and the neighbourhood of the canal, as well as from the fertility of the surrounding country. It is not doubtful that Dr. Smith obtained, by his residence in this ancient and flourishing city, and his intercourse with the well-informed and polished circles of its society, much of that accurate information respecting French affairs which plainly appears in his writings, and which, as he habitually distrusts the statements of political authorities, was the result of his own inquiries and observations.* From Toulouse they went to Geneva, where they passed two months, and then remained ten months in Paris. Here he enjoyed an intimate acquaintance with all the most eminent men of science and of letters, particularly D'Alembert, Necker, Marmontel, Helvetius, Morellet, Turgot, and above all, Quesnay, whose tastes

* He has preserved in his 'Theory of Moral Sentiments' an anecdote, which, unless it is to be found in the earlier editions, I should imagine him to have heard at Toulouse; that when the unhappy Calas was murdered by the law, and among other torments a monk was sent to obtain his confession, the wretched sufferer, already broken on the wheel exclaimed, "Can you, yourself, father, believe me guilty?" (I., 308.) The paragraph certainly is in one of the chapters which he says was altered in the edition of 1788.

and pursuits so much resembling his own formed the bond of a strong attachment. Though differing in opinion upon some fundamental points, he regarded his system as "the nearest approximation to truth that had ever been made in economical science, while the singular modesty and simplicity" of the man had a powerful attraction for so congenial a nature. He was, as is well known, only prevented by Quesnay's death from dedicating to him the 'Wealth of Nations.' It appears by a letter of Morellet, published in his Memoirs, that notwithstanding Dr. Smith's residence at Toulouse, and his intimate acquaintance with the French language, he had never so far mastered it as to speak it tolerably well; but he could, though difficultly, converse in it without much inconvenience. "Il parloit," says the Abbé, "fort mal notre langue, mais nous parlâmes théorie commerciale, banque, crédit publique, &c." As the date of 1762 is given for this acquaintance, it might be deemed that this applies to his passing through Paris in 1764, rather than his residence there in 1766; but as the Abbé mentions having seen and conversed with him repeatedly, and adds, that Turgot, as well as Helvetius, had made his acquaintance, the time referred to must have been at his return from the south; for the twelve days spent at Paris, on his way to Toulouse, could not have given time to form their acquaintance.

Upon his return to England in the autumn of 1766, he went to reside with his mother at his native town of Kirkaldy, and remained there for ten years. All the attempts of his friends in Edinburgh to draw him thither were vain; and from a kind and lively letter of Mr. Hume upon the subject, complaining that though within sight of him on the opposite side of the Frith of Forth, he could not have speech of him, it appears that no one was aware of the occupations in which those years were passed. At length, early in 1776, the mystery was explained by the appearance

of his great work—the 'Inquiry into the Nature and Causes of the Wealth of Nations;' at first published in two quarto volumes, and afterwards in three octavo. Mr. Hume lived to see it, having died in the autumn of the same year; and he immediately wrote to express his high sense of its merits, specifying accurately the chief points of its excellence—"depth, solidity, acuteness, with much illustration of curious facts;" to which, if we add the extraordinary merit of showing in what way economical reasoning should be conducted —with a constant recourse to the general principles of human nature, and a distrust of all empirical details, though with a due attention to ascertained facts of a general and not a topical or accidental class—we sum up the great services rendered to science, as well as to government and legislation generally, by this celebrated work. In regard to the originality of its views, it ranks, perhaps, less highly, as most of its doctrines had been broached by the Italian writers and the French economists, and still more by Mr. Hume in his 'Political Discourses.' We must, however, bear in mind, that Dr. Smith had begun to lecture upon those subjects as early as the year after he settled in Glasgow, that is, in 1752, when Mr. Hume's Essays were published; and in 1755 he drew up a paper containing an abstract of his doctrines, which he asserts that he taught the winter before he left Edinburgh, and consequently in 1750. As far as regards himself, therefore, we may affirm that those opinions were not borrowed from any others, but were the results of his own speculations.*

The two years immediately following the publication of his work he passed chiefly in London, and lived in the society of the persons most distinguished for political and for literary eminence. His appointment as Com-

* He says that they are contained in lectures which he had composed and had caused to be written in the hand of an amanuensis, who left his service in 1749 or the beginning of 1750.

missioner of the Customs was wholly without his solicitation, or, indeed, knowledge, until the offer was made. In 1762 the University of Glasgow had conferred upon him the degree of Doctor of Laws, and in 1788 he was chosen their Rector—an office in the gift of the students, voting by four divisions or nations. His letter of thanks on this occurrence shows how extremely gratified he was with the honour.

Upon his appointment to the Customs he settled in Edinburgh, where his venerable parent lived with him till her death in 1784, as well as his cousin, Miss Douglas, who died in 1788. These two losses sorely afflicted his gentle and affectionate heart, for he was tenderly attached to both his relations, and had never known domestic comforts but in their society. He lived hospitably, and saw much of his friends—the great lights of Scottish society in those days: Dr. Black, Dr. Hutton, Dr. Robertson, Dr. Cullen, were his chosen companions; and he took much pleasure in superintending the education of his kinsman, Mr. Douglas, afterwards Lord Strathenry, to whom he left his choice library (the only thing, as he used to say, in which he was a fop), as well as that portion of his papers which he did not destroy.

But now, although his income exceeded his wants, his far more precious time was no longer his own. The trivial but incessant duties of his office exhausted his spirits, and distracted, though they could not fix his attention. For several years he ceased to cultivate letters or science, or only gave his attention to them, as matters of amusement, and as food for conversation. He had, indeed, in the two portions of his lectures of which nothing had been published, the rich materials of works in the very highest degree interesting and important. But when we reflect that ten years had been required, and those years passed in seclusion, to systematize, to arrange, and to compose the work into which were moulded the economical part of his

lectures, we may well believe that he now, as his age declined and his infirmities increased, shrunk from performing the same office to the other portions of the lectures, when the avocations of his public duty gave a perpetual interruption to his studies. It is remarkable, too, how little, with all his great practice, he ever acquired the art of composition. He told Mr. Stewart a short time before his death, that "after all his practice, he composed as slowly and with as great difficulty as at first." Hence it naturally surprises us to learn that he never wrote, but walking about the room, dictated to an amanuensis, from which we must conclude that before he began, he had well considered the language as well as the matter, and spoke to the writer, as it were, a prepared speech.*

He began to feel the approach of age at a somewhat early period, notwithstanding the temperate, calm, and equable life which he had ever had; nor had he reached three score when he was sensible, not that his faculties, but that his bodily strength and spirits were somewhat impaired. The domestic losses to which I have adverted left him solitary and helpless; and though he bore them with an equal mind, as became a great philosopher, his health gradually declined. The immediate cause of his death, which happened in July, 1790, was a chronic obstruction in the bowels, under which he lingered for a considerable time, and suffered great pain; but he bore it with perfect resignation. When he left Edinburgh in 1773, on a journey to London, the object of which has not been explained, but which gave him the expectation of a long absence from Scotland, he wrote a letter to Mr. Hume, intrust-

* Mr. Stewart adds, that Dr. Smith mentioned Mr. Hume's facility of writing as a contrast to his own, stating "that the last vols. of his History were printed from the original copy, with a few marginal corrections." I have shown in his life, that this could not have been the case; for I have proved, both from Mr. Hume's MSS., and from his own account of his difficulty in writing, that Dr. Smith's impression was erroneous.

ing him with the charge of his papers, and intimating that, except the 'Speculative History of Astronomy,' he desired all his other writings to be destroyed, stating that they were contained in eighteen folio paper books, which were not even to be examined before destroying them. Dr. Hutton's account of what afterwards passed, coincided with this intention, and must be subjoined, as it is extremely interesting. "When he became weak and saw the approaching end of his life, he spoke to his friends again upon the subject of his papers. They entreated him to make his mind easy, as he might depend upon their fulfilling his desire. He was then satisfied, but some days afterwards finding his anxiety not entirely removed, he begged one of them to destroy the volumes immediately. This accordingly was done, and his mind was so much relieved that he was able to receive his friends in the evening with his usual complacency. They had been in use to sup with him every Sunday, and that evening there was a pretty numerous society of them. Dr. Smith not being able to sit up with them as usual, retired to bed before supper, and as he went away took leave of his friends by saying, 'I believe we must adjourn this meeting to some other place!'" He died a very few days afterwards. Mr. Riddel, an intimate friend of Dr. Smith's, who was present at one of the conversations on the subject of the manuscripts, mentioned to Mr. Stewart, in addition to Dr. Hutton's note, that he "regretted he had done so little, adding, 'I meant to have done more, and there are materials in my papers of which I could have made a great deal, but that is now out of the question.'"

In the latter period of his life, and while suffering under the illness which proved fatal, he made some important additions to his 'Theory of Moral Sentiments.' Of these, some of the most eloquent passages of his whole writings, Mr. Stewart has beautifully said,

"that the moral and serious strain which prevails through those additions, when connected with the circumstances of his declining health, adds a peculiar charm to his pathetic eloquence, and communicates a new interest, if possible, to those sublime truths, which in the academical retreat of his youth awakened the first ardours of his genius, and on which the last efforts of his mind reposed."

In 1795, a volume of posthumous works was published, consisting of four Essays. The first is a portion of the extensive work which he had begun, on the principles which lead to and direct philosophical inquiries; these he illustrates from the history of astronomy, of ancient physics, and ancient logic and metaphysics. His second is an Essay upon the imitative arts; the third on certain affinities of English and Italian verse, and the fourth on the external senses. The only part of this work that appears to be nearly finished, is the 'History of Astronomy;' but the whole of the Essays are replete with profound and ingenious views, and show an extensive and accurate acquaintance with all the branches of inductive science.

The true picture of a great author's intellectual character is presented by his writings; and of the depth, the comprehensiveness, the general accuracy of his views, on the various subjects to which his mind was bent, there can be but one opinion. His understanding was enlarged, and it was versatile; his sagacity, when he applied himself deliberately to inquiry or to discussion, was unerring; his information was extensive and correct; his fancy was rich and various; his taste, formed upon the purest models of antiquity, was simple and chaste.

His integrity was unimpeachable, and the warmth of his affections knew no chill, even when the languor of age, and the weight of ill health, was upon him; his nature was kindly in the greatest degree, and his bene-

volence was extensive, leading him to indulge in acts of private charity, pushed beyond his means, and concealed with the most scrupulous delicacy towards its objects. Stern votaries of religion have complained of his deficiencies in piety, chiefly because of his letter upon the death of his old and intimate friend Mr. Hume; but no one can read the frequent and warm allusions with which his works abound to the moral government of the world, to reliance upon the all-wise Disposer, to the hopes of a future state, and not be convinced that his mind was deeply sensible of devout impressions. Nay, even as to his estimate of Mr. Hume's character, we are clearly entitled to conclude that he regarded his friend as an exception to the rule that religion has a powerful and salutary influence on morals, because he has most forcibly stated his opinion, that whenever the principles of religion which are natural to it are not perverted or corrupted "the world justly places a double confidence in the rectitude of the religious man's behaviour."—('Mor. Sent.' I., 427.) Surely, Dr. Johnson himself could desire no stronger testimony to religion, no more severe condemnation of infidelity.*

In his simple manners, and the easy flow of his conversation, wholly without effort, often with little reflection, the carelessness of his nature often appeared; and the mistakes which he would occasionally fall into, by giving immediate vent to what occurred to him on a first impression, or a view of the subject from a single point, sometimes would furnish subject of merriment to his friends.† It was, probably, from the same simplicity and earnestness that he was apt in conversation to lay down principles and descant upon topics some-

* See 'Theory of Moral Sentiments,' Part III., chap. i., ii., and v.

† In some few instances, these traces of imperfect judgment have found a place in his works. His giving Gray the preference to almost all poets, "as equalling Milton in sublimity and Pope in eloquence and harmony," is the most singular, because the best by far of Gray's poems, the Elegy, makes no pretension to sublimity at all.—('Theory of Moral Sentiments,' I. 311.)

what in the way of a lecture; but no one found this tiresome, all feeling that it was owing to his mind being in the matter, and to his simple and unsophisticated nature. Never was there anything about him in the least like a desire to engross the conversation. On the contrary, he could sit a silent spectator of other men's gaiety, which he was perceived to enjoy even when he took no part in what excited it.

Somewhat akin to these peculiarities was his habitual absence, not only muttering in company as unconscious of their presence, but even unaware of the obstructions he might encounter while walking in the streets. One that knew him, which the sufferer did not, was a good deal amused to hear a poor old woman, whose stall he had overturned while he moved on with his hands behind his back and his head in the air, exclaim in some anger, "doating brute!"* Another was amused at the remark of an old gardener, near Kirkaldy, who only knew him by having answered his questions, somewhat incoherently put in his walks, when the 'Wealth of Nations' appeared, and he found who was its author: "Weel a weel!" quoth he, "they tell me that lad Adam Smith has put out a great book. I am sure it would be long before I would think of doing a thing of the kind." It is related by old people at Edinburgh, that while he moved through the Fishmarket in his accustomed attitude, and as if wholly unconscious of his own existence or that of others, a female of the trade exclaimed, taking him for an idiot broken loose, "Heigh! Sirs, to let the like of him be about! And yet he's weel eneugh put on" (dressed). It was often so too in society. Once during dinner at Dalkeith he broke out in a long lecture on some political matters of the day, and was bestowing a variety of severe epithets on a statesman, when he suddenly perceived his nearest relative sitting opposite, and stopt; but he

* The Scotch word is "*doited*" or "*donnert*," and expresses one whose faculties are entirely gone, if ever they existed.

was heard to go muttering, "Deil care, deil care, it's all true."

The 'Theory of Moral Sentiments,' although it be not the work by which Dr. Smith is best known, and for which he is most renowned, is yet a performance of the highest merit. The system has not, indeed, been approved by the philosophical world, and it seems liable to insuperable objections when considered even with an ordinary degree of attention, objections which never could have escaped the acuteness of its author but for the veil so easily drawn over an inquirer's eyes when directed to the weak points of his own supposed discovery. The principle or property in our nature which leads us to sympathise or feel with the feelings of others, to be pleased when our feelings are in accordance with theirs, to be displeased when they are in discord, must be on all hands admitted to exist; and thence may fairly be deduced the inference, that our approval of another's conduct is affected by the consciousness of this accord of our feelings, and our self-approval by the expectation of his feelings according with our own. But when we resolve our whole approval of his conduct and of our own into this sympathy, we evidently assume two things: first, that the accord is a sufficient ground of approbation; and, secondly, that this approbation is not independent but relative, or reflected, and rests upon either the feelings of others and upon our own speculations respecting those feelings, or upon our sympathy with those feelings, or upon both the one and the other. Now, the first of these things involves a *petitio principii*, and the second involves both a *petitio principii* and a dangerous doctrine. It cannot surely be doubted that a sense of right may exist in the mind, a disposition to pronounce a thing fit and proper, innocent or praiseworthy, unfit or unbecoming, guilty or blameworthy, without the least regard either to the feelings or the judgments of other men. It is quite

certain, that, in point of fact, we feel this sentiment of approbation or disapprobation without being in the least degree sensible of making any reference to other men's feelings, and no sympathy with them is interposed between our own sentence of approval or disapproval and its object. But it is enough to say, and it seems to answer the theory at once, that even if our sympathy were admitted to be the foundation of our approval, our inability to sympathise the ground of our disapproval, this in no way explains why we should approve because of the accord and disapprove because of the discord.

The theory, with the utmost concession that can be made to it as to the ground-work, leaves the superstructure still defective, and defective in the same degree in which the 'Theory of Utility' is defective; we are still left to seek for a reason why approval follows the perception of corresponding feelings in the one case, of general utility in the other. Dr. Paley is so sensible of this, that after resolving all questions of morals into questions of utility, he is obliged to call in the Divine Will as the ground of our doing or approving that which is found to be generally useful. Other reasoners on the same side of the question pass over the defect of their system altogether, while some try the question by assuming that we must desire or approve that which is useful; while a third class, much more consistently, consider that the approving of what is generally useful, and disapproving of what is generally hurtful, arises from the exercise of an inherent faculty or moral sense, an innate principle or property in our nature, irresistible and universal. The like defect is imputable to Dr. Smith's theory, and is only to be supplied either by Dr. Paley's method of reasoning, or by the last supposition to which I have referred. But all this concedes a great deal more than is due to the 'Theory of Sympathy,' and assumes it to stand on as good a foundation as that of 'Utility.'

Now one consideration, which has in part been anticipated, shows that such is not the case. We may sympathise with another, that is, we may feel that in his position our own inclinations would be exactly the same with those under which he appears to be acting, and yet we may equally feel that we should deserve blame, and not approval. Why? "Because," says Dr. Smith, "we take into the account also that others, that is to say, men in general, not under the influence of excitement to disturb their feelings or their judgments, will disapprove." But why should they? If they are to place themselves, as we are desired to do, in the situation of the propositus, of him whose conduct is the subject of consideration, they must each of them feel, as we do ourselves, that in his situation they would do as he is doing, or, at least, would be inclined so to do. Therefore, this appeal to others in general, this calling in the general sense to correct the individual, can have no effect upon the hypothesis; it can only exert any influence, or apply any correction, upon some other hypothesis. It appears, therefore, that in every view the theory is unsound.

At the same time, nothing can be more clear than the very high merit and the very great value of the work in which that theory is explained, illustrated, and applied.

In the *first* place, it is the first modern systematic work on ethics which exhausts the subject by going over its whole range, both as regards the principles of our nature, whereby we distinguish moral thoughts, words, and actions, and as regards the grounds of our approving or disapproving of these. The writings of his predecessors, particularly, as we have seen, those of Butler, Hutcheson, and Hume, had done much, but they had left much to be done in forming a comprehensive and general system.

Secondly. The important operation of sympathy was never before explained and traced, its effects upon our

feelings and our judgments, its sudden and even instantaneous action, its direct and indirect, and immediate and reflex workings; all the modifications which it undergoes. There remains a great body of important truth, even concerning sympathy, in the work, after we shall have deducted the portion of it which cannot be supported. Sympathy is a great agent in our moral system, though it may not be allowed either to be the only one, or one of unlimited power; and its agency was never before so fully perceived, or so clearly traced.

Thirdly. In a system of ethics the truth or falsehood of the fundamental principle is not, as in a physical or mathematical speculation, the only point to be regarded, and upon our determination respecting which the whole value of the theory depends. The exhibiting an extensive and connected view of feelings and judgments, of moral qualities and sentiments, referring the whole to one principle of convenient arrangement, and tracing their connection with each other, as well as with the common source, may be of great importance, because of great use, although the arrangement itself is defective, and the pivot on which it hinges insufficient to bear it. This merit belongs in a very eminent degree to Dr. Smith's theory, which brings together a much larger collection of moral facts, and exhibits a much greater variety of moral arguments than any other ethical treatise in ancient or modern times.

Fourthly. There are whole compartments of the work which are of inestimable value, without any regard to the theory, and independent of those portions more connected with it, of which we have admitted the value. Thus the copious and accurate and luminous account of the other systems of morals, forming the seventh part, which occupies a fourth of the book, would have been a valuable work detached from the rest. To relish it we do not require the striking contrast of perusing such works as the dry and uninteresting and indistinct histories of Enfield and Stanley. So the third section

of the first part, on the influence of success, or the event, upon our feelings and judgments of actions, what the author terms the influence of fortune, has great originality, and is at once judicious and profound. The like may be said of the fifth part, which treats of the influence of custom and fashion.

Lastly. The admirable felicity, and the inexhaustible variety of the illustrations in which the work everywhere abounds, sheds a new and a strong light upon all the most important principles of human nature; and affords an explanation of many things which are wholly independent of any theory whatever, and which deserves to be known and understood, whatever theory may obtain our assent.

The beauty of the illustrations, and the eloquence of the diction, are indeed a great merit of this work. That the author living nearly twenty years in a College, or in a small country town, and with his habits, both of study and mental absence or distraction, should have all the while been so curious an observer even of minute particulars in conduct, manners, habits, is exceedingly singular, and seems to justify a conjecture of Mr. Stewart, that he often gave a partial attention to what was passing around him, and was afterwards able to recall it by an effort of recollection, as if he had given his whole mind to it at the time. His style, indeed, is peculiarly good; his diction is always appropriate and expressive, quite natural, often picturesque, even racy and idiomatic beyond what men are apt to acquire who gather their language rather from books than from habitually hearing it spoken by the natives. Johnson, though an Englishman, has filled his 'Rambler' with very inferior English, in comparison of such passages as these: "We seldom resent our friends being at enmity with our friends, though upon that account we may sometimes affect to make an awkward quarrel with them; but we quarrel with them in good earnest, if they live in friendship with our enemies."

(Vol. I. p. 20.) "Smaller offences are always better neglected; nor is there any thing more despicable than that froward and captious humour which takes fire upon every slight occasion of quarrel." (I. 86.)

Look through the heavy and wearisome pages of the great English moralist's most admired ethical writings, his 'Rambler,' his 'Idler,' his 'Rasselas,' where will you find anything like this picture of the progress of an upstart, which, however, is in a much more balanced and sententious style than Dr. Smith generally adopts. "In a little time he generally leaves all his own friends behind him, some of the meanest of them excepted, who may perhaps condescend to become his dependants; nor does he always acquire any new ones. The pride of his new connections is as much affected at finding him their equal, as that of his old ones had been by his becoming their superior: and it requires the most obstinate and persevering modesty to atone for this mortification to either. He generally grows weary too soon, and is provoked by the sullen and suspicious pride of the one, and by the saucy contempt of the other, to treat the first with neglect and the second with petulance, till at last he grows habitually insolent, and forfeits the esteem of all." Then he concludes beautifully and truly: "He is happiest who advances more gradually to greatness; whom the public destines to every step of his preferment long before he arrives at it; in whom, upon that account, when it comes, it can excite no extravagant joy, and with regard to whom it cannot reasonably create either any jealousy in those he overtakes, or any envy in those he leaves behind." (Vol. I. p. 97.)

Here, too, is a noble passage of indignant eloquence, which I hope will not be deemed to carry with it any offence to the remote descendants of those assailed; but if it should, they can only be offended from a consciousness of the stain enduring, and that stain can be easily wiped out, so that the memory of the past shall

redound only to the glory of the present generation. He is speaking of the North American Indians. "The same contempt of death and torture prevails among all the savage nations. There is not a negro from the coast of Africa who does not in this respect possess a magnanimity which the soul of his sordid master is too often scarce capable of conceiving. Fortune never exerted more cruelly her empire on mankind, than when she subjected this nation of heroes to the refuse of the jails of Europe, to wretches who possess the virtues neither of the countries which they come from nor of those which they go to, and whose levity, brutality, and baseness, so justly expose them to the contempt of the vanquished." (Vol. II. p. 37.)

How well has he painted the man of system, and how many features of this portrait have we recognised in Mr. Bentham, and others of our day!—"He is apt to be very wise in his own conceit, and is often so enamoured with the supposed beauty of his own ideal plan of government that he cannot suffer the smallest deviation from any part of it. He goes on to establish it completely, in all its parts, without any regard either to the great interests or to the strong prejudices which may oppose it. He seems to imagine that he can arrange the different members of a great society with as much ease as the hand arranges the different pieces upon a chess-board. He does not consider that the pieces upon the chess-board have no other principle of motion beside that which the hand impresses upon them; but that in the great chess-board of human society, every single piece has a principle of action of its own, altogether different from that which the legislature might choose to impress upon it. If these two principles coincide and act in the same direction, the game of human society will go on easily and harmoniously, and is very likely to be happy and successful. If they are opposite or different, the game will go on miserably, and the society must be at all

times in the highest degree of disorder."—"For a man to insist upon establishing, and upon establishing all at once, and in spite of all opposition, anything which his own idea of policy and law may seem to require, must often be the highest degree of arrogance. It is to erect his own judgment into the supreme standard of right and wrong. It is to fancy himself the only wise and worthy man in the commonwealth, that his fellow creatures should accommodate themselves to him, and not he to them." (Vol. II. p. 110.)

There are scattered through this and Dr. Smith's other work abundant indications of the scorn in which he held faction and the spirit it engenders; but I am far from being averse to cite passages which may be supposed to reflect on my own policy and conduct, while a minister or a party chief, or to confine my quotations to those opinions with which I might be supposed more to agree. The following passage must be fairly admitted to contain much truth, though not stated in terms sufficiently measured:—"The leaders of the discontented party seldom fail to hold out some plausible plan of reformation, which they predict will not only remove the inconveniences, and relieve the distresses immediately complained of, but will prevent in all time coming any return of the like inconveniences and distresses. They often propose on this account to remodel the constitution, and to alter in some of its most essential parts that system of government under which the subjects of a great empire have enjoyed perhaps peace, security, and even glory, during the course of several centuries together. The great body of the party are commonly intoxicated with the imaginary beauty of this ideal system of which they have no experience, but which has been presented to them in all the most dazzling colours in which the eloquence of their leaders could display it. The leaders themselves, though they may originally have meant nothing but their own aggrandisement, become many

of them in time the dupes of their own sophistry, and are as eager for this great reformation as the weakest and foolishest of their followers. Even though the leaders should have preserved their own heads, as indeed they commonly do, free from this fanaticism, yet they dare not always disappoint the expectations of their followers, but are often obliged, though contrary to their principles and their conscience, to act as if they were under the common delusion." No one can doubt the truth of the conclusion to which his account of reforming schemes leads him; it is proved by constant experience, which also shows, though he leaves this out of his view, that they who refuse all reform often are the cause of excessive and perilous innovation:—"The violence of the party refusing all palliations, all temperaments, all reasonable accommodations, by requiring too much, frequently obtains nothing; and those inconveniences and distresses which with a little moderation might in a great measure have been removed and relieved, are left altogether without the hope of remedy." (Vol. II. p. 107.)

Such is the 'Theory of Moral Sentiments.' The great reputation, however, of Dr. Smith, and especially his European reputation, is founded upon the 'Wealth of Nations.' We have seen how the principles of a more sound, liberal, and rational policy in all that regards commerce and finance, had been gradually taking the place of the old and narrow views upon which all countries regulated their economical systems, and we have found the improvement begun as early as the seventeenth century. Towards the end of that, and in the earlier part of the following, the alarms of the different states which form the great European Commonwealth were so much excited by the ambition of Louis XIV., that the only subject which either interested statesmen or speculative inquirers related to questions of military and foreign policy. But the regency of a most able prince and wise ruler, profligate though his

private life might be, succeeded that splendid and mischievous reign, and the greatest, indeed the only, error of the Duke of Orleans, his confidence in a clever and unprincipled projector, however hurtful to his country for the moment, yet produced no permanent mischief, while it rather tended to encourage speculations connected with money and trade and taxation. Accordingly, both in France and Italy, those subjects occupied the attention of learned men during the first half of the eighteenth century, and we have seen how great a progress was made between 1720 and 1770 in establishing the sound principles of which a considerable portion had been anticipated nearly a hundred years before. In England, Mr. Hume had contributed more largely to the science than all the other inquirers who handled these important subjects. In France the Economists had reduced them to a system, though they mingled them with important errors, and enveloped them in a style exceedingly repulsive, and not well calculated to instruct even the few readers whom it suffered the importance of the subject matter to attract. But it remained to give a more ample exposition of the whole subject; to explain and to illustrate all the fundamental principles, many of which had been left either assumed or ill defined, and certainly not clearly laid down nor exhibited in their connexion with the other parts of the inquiry; to purge the theory of the new errors which had replaced those exploded; to expound the doctrines in a more catholic and less sectarian spirit than the followers of Quesnay displayed, and in a less detached and occasional manner than necessarily prevailed in the Essays of Hume, though from his admirably generalizing mind no series of separate discourses ever moulded themselves more readily into a system. This service of inestimable value Dr. Smith's great work rendered to science; and it likewise contained many speculations, and many deductions of fact upon the details of economical inquiry, never

before exhibited by any of his predecessors. It had also the merit of a most clear and simple style, with a copiousness of illustration, whether from facts or from imagination, attained by no other writer but Mr. Hume, unsurpassed even by him, and which might well be expected from the author of the 'Theory of Moral Sentiments.'

Analytical View of the Wealth of Nations.

I. Labour is the source of all human enjoyment; it may be even reckoned the source of all possession, because not even the property in severalty of the soil can be obtained, without some exertion to acquire and secure the possession; while labour is also required to obtain possession of its minerals, or of the produce which grows uncultivated, or the animals which are reared wild. All wealth, therefore, all objects of necessary use, of convenience, of enjoyment, are either created or fashioned, or in some way obtained, by human labour. The first inquiry then, which presents itself, relates to the powers of labour; the next to the distribution of its produce. These two subjects are treated in the *first* book of the 'Wealth of Nations,' in *eleven* chapters, to which is added a kind of appendix, called by the author a 'Digression, upon Money Prices,' or as he terms it, "the variations in the value of silver, and the variations in the real prices of commodities." The unskilful and even illogical aspect of this division is manifest; for under the head of labour, are comprehended the subjects of profit and rent as well as wages. But subject to this objection against the arrangement, and to the still more material objection which may be urged against one portion of the doctrine, the *first* book is of very great value, and unfolds at length the fundamental principles of economical science.

i. The first sub-division relating to the powers of

labour, embraces the subject of its division and its price; the former is treated of in the *three* first chapters; the latter in the *fifth* and *eighth*, but also occasionally in the *sixth*, *seventh*, and *tenth*.

1. The division of labour, both increases its productive powers, and increases the excellence of its produce. Men will work more when their attention is confined to a single operation, than when it is distracted by several, because time is saved by not passing from one thing to another, and because the power or skill of the workman is increased when he has but one thing to occupy him. But independent of his increased skill making him do more work, it makes him perform better the work which he does, and hence both the quantity is augmented, and the quality is improved of what his labour produces. The origin of this division is the principle which makes men exchange or barter their different possessions, and among these their different powers. Either one differs from another in his capacity, or each by confining his attention to a single pursuit, acquires a peculiar capacity for that pursuit. In either case, they who are differently qualified will employ themselves differently, and one will exchange the produce of his labour with the other for the produce of his; or, which is the same thing, each will work for the other, and both will thus be better served. The extent of the market will always fix a limit to the division of labour, which can have no great range in confined situations; but where it is much divided, a vast multitude of workmen will concur in producing a single article of exchange. Dr. Smith mentions the case of eighteen persons being employed in making a pin, and being thus enabled to make 86,000 pins in a day, or 4,800 each person; whereas had they worked alone, perhaps they might not have been able to make one a-piece. He adds, that the meanest individual of a civilized country uses, or commands, in some small portion at least, the labour of many thousands, and is

thus better accommodated than a savage chief, who wholly possesses 10,000 slaves, and has their lives and liberties at his disposal. Among the beneficial effects, however, of the division of labour, one is to save labour by different contrivances, and especially by the invention of machinery. This in many instances, though by no means in all, improves the quality of the article; in all cases, it increases its quantity. It therefore greatly augments the power of labour.

2. Labour is the measure of the exchangeable value of all commodities, because the possession of all is governed by labour; and in the case of exchanging one against another, the produce of the labour by which both were obtained is mutually given and received. But it is easier to compare two commodities with each other than either of them with labour as their common measure; not to mention that it is not easy to compare the different kinds of labour, as hard and easy working, skilful and unskilful, with one another. Hence, prices are generally estimated by the proportion which the commodities bear to one another. Labour is thus estimated as well as other commodities, in commodities, and its natural wages are the whole of its produce. But as each labourer seeks for employment, and as each employer is desirous of giving as little for labour as he can, therefore the competition of workmen for work enables the employer to obtain it at much less than the whole produce. When there is a superabundance of workmen and more hands than are wanted, the competition of workmen lowers their wages. When there is a scarcity of workmen and more work than hands to do it, the competition of employers raises wages. But, in all cases, except where a man labours for himself, less than the produce of the labour is paid to reward it, and the difference belongs to the employers.

It is most material to observe, *first*, that there is a tendency in the competition of workmen to lower their wages; *secondly*, that there is a point below which

wages cannot descend. Both these positions are founded on this: that the labourers are, generally speaking, persons wholly dependent on their labour. Therefore, in the *first* place, they cannot keep their labour out of the market when the demand for it is slack, as a man of property will keep his goods back when their price is low; and, in the *second* place, the labourer would cease to work if he could not earn enough to support himself in the manner in which persons of the lower order usually live, with a surplus for supporting his family, without which his race would be extinct. Hence there is a necessary connection between the wages of labour and the prices of the necessaries of life; and though the demand for work, compared with its supply, must regulate wages within certain limits, that is, between the lowest point to which they can fall and the highest to which they can rise, the latter point depends upon the demand, the former upon the cost of maintaining the labourer and his family. This will not vary with each variation of the prices of necessaries; indeed, a scarcity, by throwing hands out of employment, may even lower wages instead of raising them. But upon the average price of necessaries the amount of wages certainly does and must depend; for, if the average price is high, some proportion must be kept by wages, else the workman would either perish or emigrate, and so labour would leave the market, until its recompense became equal to the cost of living; and again, if the average is low, the competition of workmen and the increase of their numbers by the progress of population will bring down the price, that is the wages, to the level of prices; so that the average rate of wages never can be much beyond the cost of living, that is, it must fall towards the average price of the necessaries of life.

We may here stop to observe how soon we are brought, by discussing speculations on the foundations of labour and value, or real prices, to the very practi-

cal question of the Corn Laws. They who are against all legislative measures, whether for revenue or protection, that can obstruct the importation of corn, contend for the most part that their plan will lower the price of bread, though some of the most distinguished advocates of free trade in corn deny that it could produce any such effect. For my own part, I can hardly doubt, that it might in some, though no great degree, lower the average price of grain and of bread. But if it produced this effect, undoubtedly its tendency would be to lower the average rate of wages. This I say, would be its tendency; but that tendency would be counteracted by the operation of two causes, both the increased amount of the capital employed in manufacturing labour would tend to restore the rate of wages, and the extension of foreign commerce, operating upon domestic industry in all its branches, would produce the same effect; not to mention that the money rate of wages might fall, and the real rate remain the same, in consequence of living having become cheaper. I must, however, admit that the interest of the working classes in this question is not so manifest, though we should not wholly neglect it, as that of the capitalist. The main reason why the labourer has no very material interest in it, is this: In almost every state of society, indeed in every state, except that of a new and unpeopled country, the tendency above explained of the labourer to cause a glut of his only merchandise, his labour, in the market, is sure to bring down his profits, that is, his wages, to the lowest or nearly the lowest amount on which he can subsist. No change of this kind, therefore, in the national policy appears likely to effect any permanent improvement in his lot.

Hitherto we have been treating only of labour, and of matters immediately and directly connected with it; but in the remaining six chapters of the first book, Dr. Smith considers other subjects, namely, capital and its profits, or the revenue it yields, and also the man-

ner in which all exchanges are effected. As every thing is the produce of labour, as "all is the gift of industry," there may be some ground for thus reducing all within the bounds of the same book; nevertheless, these other subjects would have been more logically kept distinct from labour, inasmuch as the five chapters which we have analysed, relate to labour alone, and to labour of every kind, and labour forms the only subject of their discussion; whereas the remaining six relate to other things as well as labour, and the greater part of these discussions refer not to labour at all.

ii. The second subdivision of the book relates to the manner of effecting exchanges; and this introduces, first, the subject of money; and great part of the *fifth* chapter treats of the money price of commodities, as contradistinguished from their real price. It includes, secondly, the subject of prices. The *sixth* and *seventh* chapters treat of this.

1. The great inconvenience of traffic by barter, which made it impossible for one person to exchange any commodity with another, unless each wanted exactly the same quantity which the other had to give,—equally impossible to obtain what was wanted in one place, without sending what was redundant to the other place, —and equally impossible to obtain what was wanted at one time from a person who did not want the thing given for it at the same time,—set men upon making two inventions, the one of falling upon some commodity generally desirable, produced in moderate quantities, and capable of being easily and exactly divided into portions as well as easily transported and easily preserved, which might be exchanged for all other commodities, and thus become, as it were, a material or tangible measure of their exchangeable value, as well as an easy medium of carrying on all exchanges; the other of agreeing, that when any bargain was made for the exchange of commodities, he who did not immediately want to have the article delivered to which

he was entitled, might receive some document ascertaining his claim to receive it when he wished, and he who did not wish to part with it, but desired to have the equivalent commodity immediately, might find the document binding him to pay something for the delay, in case the other party wished it:—the former of these inventions is money; the latter is credit, or paper currency. In some rude countries shells have been used for money; in others, leather; almost universally, however, the metals have been so employed, and chiefly those which from their beauty and their scarcity, are the most valuable; gold, silver, and copper, though sometimes iron has been so used. Bills of exchange and promissory notes have greatly facilitated the operations of commerce, by enabling debts to be transferred, so that there should be no necessity of employing either goods or money to pay more than the net balance due from one given country, or from one district of the same country to another, upon the whole mutual dealings of both countries or both districts; and also by enabling credit to do the office of coin, and thus to economize the use of the precious metals. The *fifth* chapter enters at large into the subject of the coinage, and the variations, both in the actual amount of gold and silver at different times existing in the country, and in the real value of the precious metals themselves, from the varying quantities yielded by the mines of the world, and somewhat also from the variations in the demand for them; these metals being like all other commodities, liable to fluctuation from the supply and the demand varying, and their value being measured by the goods or the labour they can purchase.

2. In a rude or perfectly natural state of society, when each person enjoys the whole produce of his labour, exchange would be regulated by the time of labour, the hardness of the work, the perilous or disagreeable nature of the occupation, the skill required

to carry it on; but as society advances, when men are set to work for others and paid by their employers, there goes a part of the produce to the employer, and the consideration in the exchange or sale of the produce consists of two parts—the wages of the workman, and the profits of his employer. When the labour has been employed upon land by those who are not the owners, they must pay to the owners something for the use of it; and this is called rent, which Dr. Smith considers as entering into the price of produce, together with wages and profits, that is, the time of the labourers and the profits of the farmer; so that he considers wages alone, or wages and profits of stock alone, or wages, profits, and rent together, as entering into and composing the price of all commodities. He also considers all prices as of two kinds—the natural and the actual or market price; the former being that which replaces the wages paid for producing the article, with the profits of the employer, and in cases of agricultural produce, with the rent of the landowner also;* the other, the price as regulated by the proportion between the supply and the demand in the market, where it is exposed for sale or for barter, and which price may be either equal to, or greater, or less than the natural price.

The portion of these chapters which relates to rent is now admitted to be founded upon an erroneous view of that subject. Rent can never, properly speaking, form any part of price. It has been shown, first by Dr. Anderson in 1776, afterwards by Sir E. West and Mr. Malthus in 1812 and 14, ignorant of Dr. Anderson's discovery, that rent arises from the bringing of inferior lands into cultivation, which makes it

* It is proper to observe, that the peculiarity of rent was not wholly passed over by Dr. Smith. He expressly says, that while high or low wages and profits are the cause of high or low prices, high or low prices are the causes and not the effects of high or low rents. (Book I., chap. xi., Introduction.)

the farmer's interest to pay a consideration for the use of the better land first cultivated; so that, instead of the rent affecting the price of corn, the price of corn affects the rent; and that land is let at a rent because corn cannot be grown any longer at the same price, and not that corn is sold at the higher price because land yields a rent. The price of corn again is always regulated by the application of capital to inferior soils. It never can materially rise above or fall materially below the expense required for raising and bringing to market the corn produced on the worst soils actually cultivated. This is perhaps the most considerable step that has been made in political economy since the 'Wealth of Nations' was published.

iii. The profits of stock are the subject of the third subdivision. These vary with the wealth and property of the community as wages do, but in a very different manner; for the increase of capital, which tends to raise the rate of wages, lowers by competition the rate of profit, as indeed the rise of wages does also. The progress of the community, however, in prosperity, has a tendency to raise both the wages of labour and the profits of stock; while a retrograde state of a country never fails to lower both profits and wages. The profit of money, or interest, follow the like rules. It depends upon the proportions of borrowers to lenders; that is, on the supply of money compared with the demand for it, and the profits made by those who borrow it to invest in trade. It depends not at all upon the mere quantity of the precious metals. The profits of stock form generally the subject of the *ninth* chapter. The *tenth* relates to the rate of wages and profits in different employments, and consists of two parts—the one treating of the inequalities arising from the nature of the employment of labour or capital, the other treating of the inequalities produced by the policy of states.

1. The inequalities of the first class affecting wages

of labour are fivefold, arising severally from the disagreeable nature of the employment, the expense or difficulty of learning it, the inconstancy and precariousness of the demand, the great trust reposed in the workman or the capitalist, and the improbability of success in the work or investment; each of these disadvantages requires a certain increase of gain to the labourer, or to the capitalist, as a compensation for the disagreeableness, the education, the period of inaction, the trust, the risk of loss. Of these five circumstances two only affect the profits of stock—the agreeableness or disagreeableness of the trade, and the scarcity or risk attending it.

2. Were industry and commerce left free, these inequalities alone could affect wages and profits; but the policy of states has added to these causes of inequality several others, which disturb the natural rate of both wages and profits much more than the circumstances already enumerated.

(1.) The laws requiring several years' apprenticeship to be served before trades can be set up, prevent the free circulation of labour both from place to place, and from one profession to another. They tend to give a monopoly to both employers and capitalists, and thus to lower the wages of labour, and raise the profits of stock. The various other restrictions imposed by corporations have a like tendency.

(2.) Institutions for encouraging one kind of industry, and giving it a power greater than it naturally would possess, have the effect of drawing more to it than would naturally resort to it, and thus, from the numbers who must fail, lower the wages of labour. Free schools and colleges are liable to this imputation, which, however, Dr. Smith admits to be much corrected by the important benefits conferred if education is thus made materially better or cheaper.

(3.) The exclusive privileges of corporations produce the same effect in obstructing the free circulation

of both labour and capital from place to place, and in the same trade, which the laws of apprenticeship do in preventing the circulation of labour from place to place, and from trade to trade. The poor laws of England are shown by Dr. Smith to have the most mischievous effects on the circulation of labour, and indeed of capital also. But these have now happily ceased thus to operate, as have in all our municipal towns, except London, the exclusive privileges of corporations.

iv. The rent of land forms the subject of the *eleventh* and last chapter of the *first* book. It is not the profit of the stock vested in land, or even of that vested in its improvement, but the portion of the produce paid to the owner for the natural powers or productiveness of the soil. This subdivision consists of three parts—produce always affording rent, produce sometimes affording rent, sometimes not, and variations in the relative value of these two kinds of produce, whether compared with each other, or with other commodities.

1. The articles necessary to the food of man always enable the land on which they are raised to yield a rent, beside both supporting the labourers by wages, and replacing the cultivator's stock with a profit. The first part of the chapter enters minutely into the prices of these articles relatively, and in comparison of money or other commodities.

2. Certain articles of clothing, as wool and the skins of wild animals, articles used in building, as timber, stone, fuel as coal, some metals, all yield rent in certain situations and certain circumstances, not in others.

3. The value of articles only occasionally yielding rent will vary with that of the produce that always yields it. Some of the precious metals are dependent not on one district, but on the market of the world, from the metals being everywhere the instrument or medium of exchange. These things are to be regarded

as making their price vary, and with it the rent of the mines. As society improves the demand of the market may increase, while the produce of the mines remains the same; or the produce may increase more than the demand increases; or the produce and demand may increase together and equably. In the first case, the money price of goods will fall, and the mine become more valuable; in the second, the money price will rise, and the mine fall in value; in the third case, the money price of goods will remain stationary, and with it the value of the mine. By the value of the mine, we, of course, mean the value of the same amount of its produce in the several cases.

This leads Dr. Smith to enter at great length into the important question, how far the value of silver, the general medium of exchange in the market of the world, has varied at different periods during the four last centuries.

(1.) The first period is from 1350 to 1570, and he shows that the increased supply from the discovery of America could not have sensibly affected the value of silver during these two centuries. The progress of commerce of all kinds, internal and external, must have been the retarding cause, which prevented the influx of the additional quantity of metal from sensibly raising the money price of commodities.

(2.) From 1570 to 1640 the newly discovered mines produced their full effect in raising all the prices, and lowering the exchangeable value of silver. That effect was completed between the years 1630 and 1640. Prices had then risen to between three and four times their former rate, although the increase of commerce had increased also the demand for the metallic currency.

(3.) From 1640 to 1776 Dr. Smith does not consider that any material change has taken place in the relative value of silver and other commodities; and he examines with much particularity, and in great detail, the facts

on which the prevailing suspicion rests, and traces the progress of the supply and demand from the produce of the mines on the one hand, and the advance of society on the other. He also has occasion to show how groundless are the notions of those who regard the precious metals as constituting wealth,—that is all wealth,—when they are but a commodity valuable for use or for ornament, but still more valuable for aiding the commerce of the world as an indispensable medium of exchange.

II. The subject of the second book is stock—its nature, accumulation and employment; and it consists of five several subdivisions—the distribution of stock, the nature and use of money, the profitable employment of stock by its owners, its profitable employment by others on loan, and the various employments of stock. Each subdivision is considered in one of the five chapters into which the book is distributed.

i. The stock which any one possesses is of two kinds —that which he retains for his support, or which he has exchanged for articles of present use, and this remains in his possession, or which he takes from his revenue arising from the other portions of his stock: this is the second kind, and is used in obtaining a profit by its employment. This second kind is commonly called capital, which is of two kinds—fixed and circulating; the former (fixed) consisting of capital vested in land, or tools, or machinery, or manufactures, or shipping, which yield profit without being exchanged or parted with; the latter (circulating) being vested in goods which, to yield a profit, must be sold or exchanged. The stock of the community consists of the same two subdivisions—stock used for support, and capital or stock employed at a profit; and the national capital in the same way is either fixed,—being of four kinds—machines or instruments, buildings used for profit, improvements in land, acquired talents, useful and profitable,—or circulating, likewise of four kinds

—currency, provisions in the hands of the raisers of them, unmanufactured materials of articles of consumption, and manufactured articles of consumption. But the circulating capital of the community differs from that of an individual; because the latter is wholly excluded from his net revenue, his profits, while the former may enter into the whole trade of the community and be replaced with a profit.

ii. The only part of the circulating capital of the community which cannot be maintained without diminishing the net revenue is the money of the community. It resembles the fixed capital, *first*, in requiring like machinery an expense for keeping it up; *secondly*, in making no part of revenue; and, *thirdly*, in adding to the revenue by improvements which may economise its use. Under this head Dr. Smith discusses the principles of banking and of currency.

iii. The capital employed by the owner is distinguished by Dr. Smith into two kinds, as it puts in motion and maintains productive or unproductive labour. In this phraseology he follows the French Economists, but he differs materially from them in his classification of labour, considering as productive important branches of what they consider as unproductive. The Economists considered the labourer employed upon land as alone productive, because he alone replaces the capital and labour with their ordinary profit, and adds also a net profit; he alone replaces the cost of his subsistence, of the seed sown by him, of the tools used by him, and of the farmer's stock or capital with a profit, and also adds a net produce, the rent of the land, thus augmenting the whole capital of the community; while the retail dealer, the manufacturer, and the merchant only receive from the produce of the soil purchased with their goods, their subsistence and the profits of their capital, but make no addition to the capital of the community. Still more, they reckon unproductive the labour of professional

men and others who do not fix and realize their skill or their work in any exchangeable commodity at all. Dr. Smith shows with irresistible force of reasoning and great felicity of illustration, the great errors of this theory; and he reckons manufacturers and traders productive labourers; but then he excludes from this class all the labour of professional men. Dr. Smith's arguments on this subject are partly contained in this, the *third* chapter of the *second* book, and partly, indeed chiefly in the *ninth* chapter of the *third* book, under the head of Agricultural Systems of Political Economy. I believe it may now be safely affirmed, that his reasoning is generally admitted to be erroneous; and that as the Economists were wrong in drawing the line between productive and unproductive labour, so as to exclude that of traders and manufacturers, he is equally wrong in so drawing it as to exclude that of professional men. It is now generally admitted that the defence, the police, the government in general of a country, increasing the value of its whole capital, is as productive a labour as that of the locksmith who protects portions of the capital from pillage, or the trader who transports it from place to place; and that the efforts of those who instruct, or rationally amuse the community, give new value to its capital, which their labour enables the owner to expend in purchasing education or entertainment.

It seems now agreed that in the complicated system of civilized society, indeed in any society where the division of labour is carried to any considerable extent, it becomes wholly impossible to say who feeds, who clothes, who instructs, who defends, who amuses the community, as it is to say which of the farm servants raises the crop, or which of the artisans makes the machine or the tool; and that nothing can be more unsound than the distinction drawn between one kind of labour and another, because one realizes nothing tangible, its produce vanishing in the act of its pro-

duction, and because employing many servants or many soldiers is expensive, and employing many artisans is profitable; for what gives increased value to all capital is productive, and the employing more farm servants or more artisans than we require would be as unprofitable as employing more soldiers or servants.

These and other propositions connected with this subject, though now generally admitted, were much resisted when I first explained and defended them above forty years ago; and I shall refer the reader to an Appendix containing the principal parts of the tract then published, because it happened to be the foundation of much that has since been written on this controversy without any acknowledgement, and what is of more importance, without a due regard to the limits of the question then discussed."*

iv. Stock lent at interest is evidently capital to be replaced with a profit; but it may be used by the borrower either for his consumption, or as capital to be employed by him with a profit; and it is chiefly as capital that it is used. The profit paid to the lender is called interest and depends, like all the other profits of stock, upon the competition in the market, that is, the proportion of the lenders to the borrowers in the money market. The greater or less abundance of the precious metals, or of paper currency, has no effect upon the rate of interest; for, as Mr. Hume, who first clearly explained this subject, says, "If every man in the country were to awake one morning with double the amount of money in his coffers, all money prices would be doubled; but profits, though calculated in a different coin, would really be the same, and the profits of lenders, and of merchants, and of manufacturers would not even be nominally increased; for these profits are to be reckoned by their proportion to the capital employed in the one case, lent in the other;

* It was in No. VIII. of the 'Edinburgh Review' that the paper was published, July, 1804.

and he who before would have vested one hundred pounds either in trade or loan, would now vest two hundred pounds, and would receive ten pounds instead of the five he before received, being the very same per centage in each case." In this chapter Dr. Smith, with a very singular deviation from his general principles, regards laws regulating the rate of interest with favour, provided the legal rate be fixed a little above the market rate. This opinion has been most unanswerably exposed and refuted by Mr. Bentham, in his admirable 'Defence of Usury,' published about the time of Dr. Smith's decease.

v. The capital of a country can only be employed in one or other of these four ways—in agriculture, mines, works, fisheries; or in manufactures; or in the wholesale trade, foreign and domestic; or in the retail trade. Dr. Smith considers it clear, that agriculture puts in motion most productive labourers, manufacturers next to agriculture, then retail trade, and wholesale trade least of all. He also holds that agriculture augments the capital of the community most rapidly, manufactures next, then retail trade, and lastly wholesale. The wholesale trade he divides into three branches, properly speaking into four—the home trade, the foreign direct trade of consumption, the foreign indirect or round about trade of consumption, and the carrying trade. The first he considers the most beneficial employment of capital, because it replaces two national capitals; the second and third are, according to him, less beneficial, because they replace one national and one foreign capital; while the carrying trade replaces two capitals, both foreign. I believe the views contained in this chapter are pretty generally admitted to be erroneous, that is to say, as regards the relative importance assigned to different branches of trade or employments of capital. This seems as regards the comparison of agriculture, manufactures, and trade, to follow, from what has been stated under the

third subdivision of this subject, and from what is more fully explained in the Appendix. In truth Dr. Smith here, as elsewhere, while he differs with the Economists, falls into some of their most erroneous views. He regards agriculture as wholly different from manufactures, because nature here works with man, and adds to the amount of his possessions. But the powers of nature are as much required to aid us in a chemical, nay, even in a mechanical process, as in agriculture. The fermentation of grains to distil a beer or a spirit from them is as much an operation of nature as the germination of the seeds to grow the crop; it is as impossible for man to augment the quantity of matter in tilling the ground, as in working up the produce; all he does in either case is to new-mould, and to fashion; and the rude produce is as useless before he manufactures it, as the water, the salts, and the gaseous bodies, of which vegetables consist, are useless before the process of vegetation. The difference in trades which replace foreign, and those which replace home capitals, is better founded, although the sounder view is to consider all nations which interchange each other's commodities as one great community, and to regard the gain of each, even by the labour which the capital of any other puts in motion, and by the accumulation of profits which thence arises, as the gain more or less directly of that other; thus extending the doctrine of the division of labour to the whole community of nations, upon which doctrine we have seen depends the refutation of the errors respecting productive and unproductive labour in the case of any one nation.

III. The different progress of wealth in different nations forms the subject of the *third* book, which therefore treats in four successive chapters, *first*, of the national progress of opulence, by the cultivation of the country, and then by the improvement of the towns; next of foreign commerce, as capital is safer in the first than the second, and in the second than in

the third employment. *Secondly*, the various discouragements to agriculture by the circumstances and the barbarous policy of the European states after the fall of the Roman empire. *Thirdly*, the rise and progress of the towns in the dark ages. *Fourthly*, the improvement promoted in the country by the progress of the towns, which gave the agriculturist an increased market for his produce, applied their capital to the improvement and purchase of his land, and introduced a more regular police, as well as a freer state of society generally.

IV. The fourth book, the most important of the 'Wealth of Nations,' is devoted to the consideration of the two great systems of political economy, the Mercantile and the Agricultural; the discussion of the former occupies eight chapters, and one-fourth part of the whole work; that of the latter is comprised in a single chapter of moderate extent.

Part I. This elaborate, most able, and most completely satisfactory inquiry commences with showing the popular mistake or confusion which lies at the bottom of the mercantile system, runs through its whole doctrines, and gives rise to all its practical applications, that gold and silver, being the instruments of exchange and the ordinary measures of value, are therefore wealth itself independent of their value as instruments and measures, and that the great object of statesmen should be to multiply them in any given country, in order thereby to increase that country's wealth. Rulers having begun upon this view, prohibited the exportation of the precious metals; but this was found most vexatious to commerce, and therefore the traders urged the governments of different countries to suffer the exportation, by which goods might be obtained, the re-exportation of which would restore with a profit the specie that had been sent to buy these, and thus augment its whole mass. These merchants, however, wholly adopting the fundamental

error, and regarding specie as alone constituting wealth, further urged that the direct prohibition to export them could scarce ever be effectual, on account of the small bulk of the metals and their easy smuggling, the evil of evading the law adding to the cost of getting the metal; but they represented the true policy to consist in so regulating the balance of trade, as to make the exports exceed the imports of goods generally, the difference being of course paid in gold and silver. These arguments prevailed generally, both with speculative men and with practical statesmen; the home-trade, by far the most important of all in every country, was undervalued; foreign commerce was regarded as the great source of wealth; and positive restraints were imposed upon importation, while direct encouragements were given to exportation. The restraints were of two kinds,—restraints upon foreign goods, which were or could be manufactured at home, and this was a restraint on trade in these particular commodities with all countries indiscriminately—and restraints upon almost all goods from countries with which the balance of trade was supposed unfavourable. Encouragement to exportation was given in four ways, by drawbacks of the excise imports, or certain duties imposed; by actual bounties on exportation or on home manufactures, by treatise of commerce to obtain commercial privileges or favours, by planting colonies and monopolizing their trade. These are the six grand resources of the Mercantile System—its great expedients for obtaining an increase of the precious metals by making the country export much and import little. Accustomed as we now are to the plain and obvious consideration, that those metals, like all other merchandise, can only be bought with other merchandise, that when this merchandise exists, it will obtain the metals; that unless it exists none can by any means be procured; that the natural industry of the country can alone give it existence; that this industry, if cramped by regulations,

can never raise it so cheaply or so profitably as when left free; that all restraints upon importation diminish the value of home produce by raising the price of the foreign, which is its price; that all bounties are a waste of the capital, and obstruct the very ends they are intended to gain; finally, that the metals themselves are not wealth, but only one part, and a very small and most insignificant part, of the national capital, which might be augmented to exuberance, and make the nation abundantly and superabundantly wealthy, without any specie at all, if means could be devised of restraining the excessive issue of a paper currency, or any other instrument could be devised for conveniently effecting exchanges—accustomed as we now are to these obvious views of this subject, we seem to wonder that the elaborate exposure of manifest error to which the *six* chapters of Dr. Smith's work are devoted, each chapter examining one of the resources of the mercantile system, should ever have been required in order to overthrow the fabric. But it is because he wrote those invaluable chapters that these doctrines, which though often before attacked, as we have seen, both abroad and at home, yet continued everywhere to prevail, and especially to prevail among the rulers of the world, at length received their demonstrative refutation, and that we now can look back with wonder on the darkness which this great light dispelled.

i. If the importation of any commodity is restricted, there is an inducement held out to the raising or the manufacturing of that commodity at home; capital is drawn towards its production which would not otherwise have been so employed, and workmen are engaged in raising or manufacturing it who would have been otherwise occupied. But this is hurtful on two accounts; men's regard for their own interest is sure to make them work and employ workmen in the way most likely to yield them a profit; and the natural advantages of each country or district of a country for raising or

for manufacturing certain commodities must always determine where they can be grown or made the cheapest. The inducing men to cultivate one branch rather than another of industry, must therefore prevent their industry from being most profitably employed, and the confining the inhabitants of the country to the commodities produced by its own inhabitants makes them pay dearer for them than they otherwise would do; and thus lowers the real value of all the other produce of the country.—Dr. Smith states the exceptions to which the general rule is liable. They are said by him to be two-fold, but in reality he allows four exceptions. Defence being more important than wealth, he greatly praises the provisions of the 'Navigation Law,' whereby, in order to increase the amount of British shipping, and to destroy the carrying trade of Holland, none but British ships could be employed either in the colonial, or the coasting, or the carrying trade, or in importing from any foreign country any article not the produce of that country, also prohibiting British ships to import from one country the produce of any other.—Again: when any tax is laid upon one article of home-growth or manufacture, he considers it right to lay an equal or countervailing duty upon the importation of the same article.—He also allows that when any article has been unnaturally encouraged by former prohibitions, or by the restriction of importation, justice, as well as policy, requires that the prohibition or restriction should only be taken off "slowly, gradually, and after a very long warning."—Finally, he conceives it just and right to retaliate on Foreign States, which have restricted the dealing in our commodities by restraining our people from dealing in theirs, providing we can thus hope to obtain an alteration in their policy. But the consideration how far such experiments are likely in any case to succeed, he says, belongs not so much to the philosopher or the lawgiver as to him whom he is pleased to mention as the "insidious and

crafty animal, vulgarly called a statesman or politician, whose councils are directed by the momentary fluctuation of affairs." (Vol. II. p. 201.) I trust I may be excused for saying that my councils were always directed by more liberal and permanent views than Dr. Smith himself on this one point entertained; being always pointed to dissuade my "brother animals" from any such retaliating process as he approves, and to recommend liberal principles as more likely in the end to remove the prejudices of Foreign States. In one thing we all appeared quite to agree with Dr. Smith, that "to expect the entire restoration of freedom of trade would be as absurd as expecting to see an Oceana or an Utopia established." (Vol. II. p. 206.)

ii. The unreasonableness of general restraints upon importations from particular countries on account of the balance of trade is next shown, first, on the principles of the Mercantile System, and secondly, upon general and sounder principles.

1. Supposing that the freest trade were allowed with any given country with which the balance was supposed unfavourable, it by no means follows that this would prevent a gain with all countries in the amount of specie imported, because the importation of more goods from the given country than we exported to it might very possibly enable us to export more to some other countries with which we had no other means of trading, because even if all the goods imported from the given country were consumed, and not re-exported, the balance would be better preserved if they were bought cheaper there than they could be elsewhere. Add to this, the impossibility of ascertaining with any tolerable approach to accuracy the balance of trade with any country from the inaccurate valuations in custom-house books, and from the course of exchange being influenced, not merely by the dealings between any two countries, but by the dealings of each with all other countries, as well as by the state of the

coin in both, by the arrangements made for defraying the expense of coinage, and by the practice of paying sometimes in bank money and sometimes in specie currency. The course of exchange will frequently appear to be in favour of nations which pay in bank money, and against those which pay in currency, though the real exchange may be the other way in each case. This leads to a long but very valuable digression concerning Banks of Deposit, especially that of Amsterdam, on which the author tell us, in the last edition, that he received his information from Mr. Hope; and it was the first time that any intelligible account of that celebrated establishment had ever been given to the world. Mr. Hope estimated the amount of the deposits in 1750, at about three and a quarter millions sterling; and Dr. Smith, like the rest of mankind, believed that the oath annually taken by the burgomasters was sacred "among that sober and religious people," and that not a florin was ever issued except to the depositors, the whole profit of the bank being the commission of a quarter per cent. on deposits of silver, and a half per cent. on those of gold. But about the very time that Mr. Hope spoke of, or immediately after, the faith which had remained inviolate from 1609, the date of the Bank's foundation, was broken by that body,—large loans were secretly made to the Government and the East India Company; the annual oath continued to be taken by that "sober and religious people," and to be annually broken; in 1790, the bank announced that no deposits under £250 would be returned, and that ten per cent. would be returned on all others; and all this was submitted to without impairing the bank's credit—so sturdy a plant is confidence, grounded on long habit and long-sustained good faith! At length, in 1796, it was discovered that above a million sterling, lent covertly, could not now be recovered from the State by the Company, whose claims on the public

were assigned over to its creditors. The bank paper, before bearing a premium of 5 per cent., now fell to 16 discount.

2. Hitherto we have tried the merits of the Mercantile System for increasing the precious metals, on the principles of the system itself. But more rational views condemn the attempt altogether. The supposition that two nations can only gain by trade when each imports an equal value of commodities from the other, and that if one imports more, it loses, is perfectly absurd, and betokens a complete inattention to the nature of trade as well as of money. If both import from each other an equal value of goods, so far from neither gaining, both gain, and nearly in an equal degree. The benefit of England in receiving the wines of France, which it cannot grow, is equal to the benefit of France in receiving from England the coal, which it cannot raise, or the steam-engines, which it cannot make. If there were no balance at all on the year's account, not only all the coal and machinery, but all the marketable goods in England would be the more valuable in amount, because all could be exchanged for wines, and not only all the wines, but all the silks and other goods of France would be more valuable, because they could be exchanged for our coals or our engines. The interest of each nation is to obtain a vent for the produce which it has no occasion for, and a supply of the things which it wants. Its labour and its capital is thus most profitably employed; its comforts are provided for, and its wealth is increased. If it can buy cheaper than it can raise or make, it is more profitably employed in importing than in producing, for the very same reason that it is more profitable for the farmer to buy his ploughs and his clothes than to make them. Where it can buy cheapest and sell dearest, there ought it to resort—for the very same reason that it is more profitable for a farmer to buy of the workman in the next parish who makes ploughs

or clothes better and cheaper than the workman in his own parish. The only balance to be considered by rational men as affecting the progress of any nation's riches, is that of production and consumption: when it consumes more goods than it produces, it will be impoverished; when it consumes less, it will be enriched by accumulation. But this accumulation will be going on, and the national wealth be increasing, while the exportation of specie by the balance of trade is going on during the whole time. For half a century together this appears to have gone on in the North American States before the Revolution; and yet, though the currency was almost entirely paper, no part of the world had made greater or more rapid strides towards wealth and prosperity.

iii. It does not appear that drawbacks are exposed to serious objections upon any principle. If any commodity is taxed at home, and cannot be re-exported with the weight of the duty upon it, there seems no reason why the whole or the greater part of the tax or duty should not be repaid upon the exportation. Care must, of course, be taken to prevent clandestinely retaining or re-landing the goods for home consumption; and Dr. Smith considers the exportation to our colonies, which can only receive goods through us, as not a case for drawback, because the impost must be paid by the colonists, if they want the goods.

iv. Bounties stand in a very different predicament, if we take care to distinguish between real and only apparent bounties. A real bounty is the payment of something to encourage the exportation of goods not subject to any such impost at home. An apparent bounty is the payment of something to encourage the exportation of goods which are either directly subject to a tax, or made of, or with, articles subject to a tax—as refined sugar made of taxed raw sugar, or gunpowder made of saltpetre that has paid duty. These apparent bounties are, in reality, drawbacks, and fall

within the exception of the last subdivison. But real bounties are, in every case, objectionable; they are liable to the general objection urged against encouraging one branch of industry, or one employment of capital, by restricting importation; they force labour and capital into employments they would not naturally seek, and therefore would not advantageously have. But they are liable to the still greater objection, that the giving them always assumes the employment of capital to be prejudicial, the trade to be a losing one, else there could be no reason whatever for giving them; and thus we pay more for driving a losing trade, and wisely make a present to foreigners at the expense of our own people, for the purpose of increasing the amount of the specie which we are to gain from those foreigners. Dr. Smith examines particularly the two most celebrated cases of bounty; first, that on exported corn, which he shows to have both raised its price to the public at the public expense—to have prevented the plenty of one year from providing for the want of another—to have had no effect in encouraging tillage, because it only gave the grower a nominal benefit—to have raised the money price of our goods in the home market, and lowered their price abroad —to have enabled foreigners to eat of corn somewhat cheaper than we do ourselves. The other bounty discussed is that in the herring and whale fisheries; in which he clearly shows the Government to have been grievously imposed upon by the great authors of all such measures—the members of the commercial interest, whom he never spares in his sharp and severe censures.

To this subdivision is naturally enough added a dissertation called, somewhat inaccurately, a 'Digression on the Corn Trade and Corn Laws,' the bounty having been already touched upon. There are four trades engaged in this line of business—those of the inland dealer, the importer, the exporter, and the

carrier or importer for re-exportation. These trades may be carried on separately or together.

1. The interest of the consumer, as well as of the producer, is clearly served by the first class of traders; nor can anything be more clear than that, where they raise the price, which they have no power of doing unless there is a scarcity either begun or impending, they benefit the people by putting them on short allowance, and preventing dearth from being exchanged for famine. The gross injustice, and revolting absurdity, of all the laws now happily abrogated, against forestalling and regrating, intended to keep down prices but in reality keeping them up, by discouraging trade, by discouraging agriculture, and by discouraging thrift, it is needless to illustrate either by reason or example.

2. The trade of the importer is likewise beneficial to the community by somewhat lowering the price of corn; and though this may lower the nominal revenue of the home producer, it likewise lessens his expenses, and so leaves his net income the same, not to mention that in common years there is never much more than the six hundreth part of our consumption imported from abroad. One thing, however, requires to be observed as to the admission of foreign corn. The producers have for a long course of years received a money income higher than a free trade in grain might leave it. Hence the difficulty of reducing that income, when all their settlements, and all their mortgages, and all their other time bargains, as well as the rents paid by their tenants on existing leases, have been calculated and augmented upon the foot of higher prices. The importance of the landed interest to any country is not easily overrated. Dr. Smith himself, on every occasion, puts it much higher than that of any other of the great classes of the community. In a form of government, and frame of society, such as ours, it is to be carefully considered. The burthens peculiar to the owners and cultivators of the soil are likewise to

be taken into the account. Not only do they pay a heavy land tax, but still heavier county and parish rates, amounting in all to between six and seven millions. Supposing that the malt tax falls wholly on the consumer, yet it certainly tends to discourage the cultivation of barley very materially by diminishing its natural consumption. Barley too, is the grain to which soils are more universally adapted than to wheat; and, independent of the direct operation of the tax in discouraging its growth for the sake of revenue, the regulations necessary to prevent illicit distillation press severely on the grower by preventing him from using grain to feed his cattle. All these considerations made the late Mr. Ricardo, a strong and unsparing advocate of free trade, propose a permanent fixed duty on corn imported, as a compensation to the farmer, in respect of his being pressed by burthens from which the foreign grower is free.* Hence, too, some reasoners extend several of Dr. Smith's arguments in favour of countervailing duties, and his view of further exceptions being allowed to the rule of free importation by the consideration that other things may be more important than wealth, and, possibly, that the support of the internal institutions may be as much a fair object of care as its external defence of a country. On this inquiry I do not enter. The subject of steadiness of price is not considered by Dr. Smith, though

* The argument often so thoughtlessly employed by the wild adversaries of the landed interest, that the poor rates fall on houses, and thus on the merchant and manufacturer as well as on the landowner and farmer, seems quite inconceivable. Suppose them right in stating that half the poor rates fall on house-rent, still, as the landowner and farmer pay this also, there would remain above three millions exclusively laid on them. No man of common reflection can be ignorant that the manufacturer is rated at the rent of a building worth to him, perhaps, £20,000 a-year, that rent being £1,000 or £1,200, while the landowner whose income is the same pays in the proportion ten or twelve times more. It is equally inaccurate to reckon the excise, customs, stamps, as burthens falling on the rest of the community and not on the land. The landowner pays his share of these largely, and the stamps are peculiarly burthensome to him.

it forms, at least in our times, the main topic of those who defend the corn laws. The tendency of the importation, by opening our market to the growers of Poland and the Ukraine, though not in terms referred to, must have been in his eye, because in no other way could the free importation of corn permanently reduce its price, the opening of our markets having the inevitable effect of raising its price abroad. But as Poland and the Ukraine can only increase their production of grain gradually in the gradual advance of their population, it seems evident that the permanent fall in prices must be the work of time, and could not easily occasion any great or sudden shock to our internal system.

3. The free export of corn, whether home-grown or imported, is essential both to the interests of the producer and the consumer, because, unless it is certain that the quantity grown, if superabundant, can be easily taken off, the growth will be pared down to so low an amount as must prevent cheapness, and, unless it is certain that any surplus imported can be re-exported, there will be the same slowness to lower prices by importation. As for the arguments against importing or exporting for fear foreign States should shut their ports and we should thus lose our needful supplies, the experience even of Dr. Smith's age showed how little ground there was for such alarms; but in our day, who have seen one vast system of continental despotism established upon a monstrous military power, wielded by a single man, and wielded in direct hostility to our commerce, yet fail to prevent a much greater importation than usual of all kinds of grain, anything more chimerical than such fears cannot well be imagined.

4. The carrying trade is not perhaps of so much importance to the home market as the three other branches of the corn trade: yet it does contribute to its supply; for the carrier will always be ready to keep

part of his capital under his eye and controul, and thus to sell at home, just as Holland became a great emporium of all articles, while she was the carrier of the world.

The general soundness of Dr. Smith's views upon this important subject has never been questioned by persons of good authority, unless upon the questions connected with the bounty. Some writers, who are in general the advocates of free trade, have considered the benefits conferred by the bounty upon agriculture, and through agriculture upon the whole industry of the community, to be sufficiently important to counterbalance the arguments against so great a deviation from all sound principle as the payment of a portion out of the national capital, for the purpose of drawing more of this capital into one line of employment than would otherwise seek that line. They have also considered that a reduction in the price of agricultural produce is the ultimate effect of this system. Dr. Anderson, the author of the true Theory of Rent, (as far back as 1777,) and Mr. Malthus hold these opinions. Others, again, who entirely agree in Dr. Smith's opinion, dispute the reasons by which he supports it. Thus Professor Maculloch has shown that there is a fallacy in the assumption of the real value of corn being unalterable as Dr. Smith supposes, (Corn Laws, 'Encyclopædia Brit.' VII. 347.) And Mr. Horner, in a most able paper in the 'Edinburgh Review' (V. 199), shows that Dr. Smith arrives at the conclusion of the enhancement of price in the home market by a wrong route, the enhancement being by him regarded as the direct and inevitable effect of the bounty, and kept separate, from its effect in extending the foreign demand, whereas Mr. Horner shows, I think very clearly, that the extension is the direct and main cause of the enhancement, and that the bounty only operates incidentally in this way. It is also to be observed that no reference is made to the operation of the bounty upon the foreign

demand in the two first editions of the 'Wealth of Nations.' It may be further mentioned that, some time before the 'Wealth of Nations' was published, an act had passed materially relaxing the bounty law of King William. Of this alteration Dr. Smith remarks, that like the laws of Solon, if not the best it was as good as the temper of the times would admit; and it is well known that Mr. Burke, its author, told him, when objecting to it, that although philosophers had the privilege of conceiving their diagrams in geometric accuracy, the engineer must often impair the symmetry as well as simplicity of his machine, to overcome the irregularities of friction and resistance. The corn bounty was entirely abrogated in 1815; and in 1830 all bounties whatever were repealed.

v. The subject of commercial treaties is next to be considered. They are liable and always to this objection, that as they grant advantages to the growers or manufacturers of one nation over the growers and manufacturers of all others, so those advantages are at the expense of the people living under the Government which has granted them. They buy dearer and sell cheaper than they would do if their trade was left free with all nations. No loss will be incurred either by the nation or by individuals as in the case of bounties, but a smaller gain will be made than might otherwise have been made. Unless some gain were made, the monopoly given to the foreigner would extinguish the home trade. But some commercial treaties have been made, with the view of turning the balance in one country's favour with the other country to which it gave a monopoly of its markets. An instance of this is given in the Methuen Treaty, in 1703, with Portugal, examined in detail by Dr. Smith; who shows that the obligation incurred by Great Britain to admit Portugal wines at a third part less duty than French, in return for Portugal only agreeing not to raise the duties on British woollens, though receiving them on the same

terms as those of Holland and France, is an unfair and improvident bargain, even upon the principles of the mercantile system, of which this treaty is vaunted as the especial triumph and glory. The great aim of that system, to increase the amount by importation of the precious metals, undoubtedly gave rise to this treaty with Portugal, whose share in the mines of gold is so large. Dr. Smith takes occasion to show, that there needs no care whatever of the Government in any country to obtain these metals, whether for trade, or for revenue, or for subsidy, or for any other head of expenditure, foreign or domestic, as its ordinary commerce must always insure a sufficient supply of them; that is, as much of them as it can afford to pay for, and this is as much as it ever can have.—He takes occasion likewise in closing this subject to introduce a discussion on the coinage and in favour of a moderate seignorage, a discussion out of place in this part of his work, and which rather belonged, as he himself admits, to the subdivision of the first book which treated of money. Perhaps it more properly should have formed another head of the expedients of the mercantile system. In its present place it seems much more entitled to the name of a digression than any one of the three which have been so termed, with this difference, that it has no kind of connexion with the subject to which it is annexed, and can hardly, like those others, have been suggested by it, excepting that it follows the remarks on Portuguese gold.

vi. The great subject of Colonial establishments concludes this discussion of the expedients of the commercial system. Dr. Smith *first* explains the motives for planting new colonies; *secondly*, the causes of their prosperity; *thirdly*, the advantages which Europe has derived from the discovery of America, and the easier communication by sea with India.

1. The ancient colonies of Greece and Rome were suggested by different circumstances, and founded on

different principles. Their names sufficiently show this diversity. The Greek settlement was called, *αποικια*, a going from home; the Latin, *colonia*, a plantation; the former kinds of colony lost all connexion with the parent state; the latter were its advanced posts or garrisons in a conquered country; both originated or at least had some connexion with the narrowness of the home territory, and the necessity of obtaining settlements elsewhere. With the Greeks, no other purpose was served but to get rid of their surplus population; with the Romans, beside this, the securing their conquests formed a motive for colonising. The modern colonies had some concern with the convenience of emigration, but far more with the promotion of commerce and the extension of dominion. After the Venetians and Portuguese had enriched themselves by the East Indian commerce, the Spaniards and Portuguese turned themselves to exploring and settling the islands and continent of South America, where the rich returns of gold and silver gave them so great commercial renown, that England, France, and Holland pursued a like course, and planted colonies in the American islands and continents. The jealousy with which Spain and Portugal prevented all foreign intercourse with their colonies made it necessary for other countries to obtain similar possessions, if they would have any trade in the valuable produce of those distant fertile countries; and each nation successively founded its colonial policy upon the same jealous and exclusive spirit which had shut them all out of the colonies first established. The motive of all these colonizing projects was the thirst of gold; in all of them the traffic in other produce was soon found to be the most valuable; and the commerce in commodities at first despised, gives rise now to the bulk of the European intercourse with the new world.

2. The abundance of good land, and the knowledge of agriculture and the arts which settlers take out with them to a new or a conquered colony, are the causes

of its rapid increase in population and in wealth. The American plantations greatly surpass the Greek in this respect, and very greatly surpass the Roman, while their distance from the mother country gives them far greater freedom than the latter had in managing their own concerns. Even under the tyrannical government and bad management of Spain, Mexico had 100,000 inhabitants a century ago, five times as many as at the conquest. Brazil had above half a million of Portuguese, or their descendants; while in British North America, the number of the people doubles in seventeen or eighteen years, and now amounts to nearly 20,000,000. The more rapid progress of our colonies is owing to four leading circumstances: the law preventing land from being engrossed in a few hands, and preventing it being conveyed unless a certain portion is cultivated; the general law of equal division by succession, without regard to primogeniture; the low amount of the taxes; the more favourable trading system, which gives no exclusive companies the monopoly of their commerce, and allows certain produce to be freely imported into the mother country, throwing open for all produce all her ports, and giving them all the inestimable advantages of a free and popular government.

3. The advantages derived from the colonies have been either those obtained by Europe at large, or those obtained by the several colonizing Powers.

(1.) The comforts and enjoyments of life have been varied and increased to all nations in the old world. The industry of all has been stimulated by the new vent for their produce, and countries which even do not directly trade with the colonies, have benefited by their produce, and by the surplus produce of the countries that conduct the trade, which is occasioned by the colonial demand.

(2.) The colonizing countries have derived not only the benefit which all States receive from their own dominions, but also the peculiar advantages of their

exclusive traffic with the colonies. The former have been very trifling, as means of defence and revenue are all that a State can derive from its own territory, and of these nothing has been afforded, except the revenue derived from the Spanish and Portuguese settlements. But the commercial monopoly has certainly been very lucrative. This advantage, however, is, by Dr. Smith, considered to be rather relative than absolute,—an advantage over nations having no colonies, and whose industry is to a certain degree oppressed by their exclusion from the colonial commerce. The monopoly has kept down the agriculture and trade of the colonies, and thus it has injured the mother country by curtailing the natural supply and thereby raising the natural price of colonial produce. But it has also injured the natural trade and agriculture of the mother country, by drawing much more capital towards the colonial traffic and cultivation than would naturally have gone thither, thus gradually lowering the profits by increasing the competition in the colonial trade, and proportionably decreasing the competition and raising the profits in other branches of commerce. The rate of profit in the mother country being thus kept unnaturally high, has necessarily been hurtful to its trade with all other countries. Dr. Smith likewise contends, that the monopoly draws capital from a foreign trade of consumption with foreign countries yielding quick returns, to a similar trade with distant countries yielding slow returns; that it draws capital from a direct to a round-about foreign trade of consumption; and that it draws some capital from all trade of consumption to a carrying trade. In these respects he holds the colonial monopoly to have been greatly prejudicial. Lastly, he considers it a disadvantage that this great branch of commerce occasions our manufactures not to be adapted to a variety of small markets but to one or two large ones, destroying the uniform and equal balance that would naturally have taken place among

the different employments of capital, and thus diminishing the great security derived from a moderate amount of capital being invested in a great number of trades, of which if one should fail, another may be expected to succeed.

It is not to be denied, that a great portion of Dr. Smith's objections to the colonial monopoly are well founded. The object of that monopoly is to overcome the natural effects of distance and severance, and to render the remote territory, situated at the other extremity of the globe, a portion of the mother country's European dominions. But even if such be its object, it is treating the colony unlike any other part of the parent State's dominions, to forbid all trade between its people and foreign States, and to confine its commercial existence to its relations with the rest of the empire. No one ever thought of compelling Lancashire or Devonshire to trade with the other parts of England alone. But we have even gone further and prohibited certain of our colonies from trading with some of our other colonies, as if Lancashire and Devonshire should be obliged to trade with Middlesex alone. However, it must be allowed, that Dr. Smith is wholly in error when he regards the colonial trade and agriculture as foreign, and the capital invested in them as invested in remote foreign trade, round-about foreign trade, and carrying trade. The colonies are part of the empire; their people are its citizens and subjects; the trade with the colonies is as much a home trade, as much replaces British capital, and puts in motion two classes of British labourers, as the trade between two provinces of the mother country. Indeed it resembles most nearly the commerce between the country and the towns in any given state, the traffic of the producers with the consumers, of the farmers with the manufacturers, of all commerce the most gainful. It is also certain, that he has overlooked another and a most material consideration. The capital invested in foreign agriculture

where the capitalist and his family reside on their property or their farms, remains abroad, both stock and profits. The capital invested in colonial agriculture returns its profits almost immediately to support families residing in the mother country. These profits, moreover, can be subjected to the taxation of the State with a view to support its revenue.

The benefits of the colonial trade, and even its monopoly, in contributing to the naval resources of the State, have been freely admitted by Dr. Smith, as has already been seen. But one important consideration he has wholly left out of view, or only vaguely hinted at it. When comparing the effects of the colonial trade as monopolized with its effects if left free, he assumes that all nations have their colonial trade unfettered, and omits to remark that any one doing so would not gain at all as he supposes, if the others continued the exclusive system.—Akin to this is his overlooking the dilemma in which England, France, and Holland were severally placed by the Spanish and Portuguese monopolies. In order to share the advantages of the colonial trade they were compelled to have colonies of their own. It is one thing to ask, Whether there be any benefit from this or that given country planting colonies? and another to ask, Whether the colonial trade is ever otherwise than in some degree beneficial? Possibly it would be better if two or three nations should plant colonies, especially if they let others profit by their traffic, that these others should have none of their own. But who is so wild as to expect that ever this could happen, that any nation should be at all the expense, trouble, risk of founding and rearing a settlement, and afterwards of governing and protecting it, and then let all other nations benefit equally by its commerce?—Lastly, Dr. Smith has omitted to consider the great advantage which a nation derives from having once had colonial possessions, even after they have thrown off the yoke and ceased to be under the government

of the mother country. The market for her produce is thus continued; the intercourse of emigration and of trade is maintained between the nations now become independent; common origin, common language, common laws and customs, making the firm bond which naturally exists between the parent state and the colony, survive their political severance; and if no untoward circumstances have attended that event, there must always remain a natural amity and alliance between the two branches of the same people. All these things have been fully explained in the work upon Colonial Policy which I published two-and-forty years ago, and they are there illustrated by the history of all the European settlements in America and elsewhere. It is also there shown how little the charge of colonial government has been, and how rarely colonial interests have involved the mother country in war.

vii. The subject of the mercantile system, the first part of the fourth book, is closed with a general chapter, containing not a summary of the insuperable objections to that theory, as might have been expected from the title—'Conclusion of the Mercantile System'—but a number of remarks on bounties and prohibitions, specifying those actually given or imposed. These it is unnecessary to abstract.

In concluding the analysis of this, the most important part of Dr. Smith's work, we may be permitted to consider, with some regret, that he should have so constantly expressed himself with harshness respecting the mercantile and manufacturing classes of the community, or rather the merchants and the master manufacturers. He, on all occasions, regards them as inferior in character to the land-owners and farmers, inferior in patriotism and disinterestedness, inferior in good feeling—in short only to be praised for their greater acuteness, and better knowledge of their own interests. This spirit, which he derives from a view of the many restrictive laws which may no doubt be traced to them,

breaks forth constantly in the course of the book, but it is especially to be observed in such passages as that of Book iv., chap. ii., (Vol. II., p. 307); Book iv., chap. vii., (II., 441); Book iv., chap. viii., (II., 489*). He carries his prejudice even further; he regards manufacturing industry as wholly unfavourable to both the acquisition of knowledge, the enlargement of the mind, and even the enjoyment of health.

Part II.—The remaining part of this fourth book is devoted to a full explanation of the agricultural system, that is, the theory of the French Economists, and to remarks tending to show how erroneously it deals with the classification of labour and profits, when it represents employment of labour or of capital in agriculture as alone productive. The subject has already been so fully discussed, both in the foregoing analysis and in the Appendix, that nothing remains to be added in this place.

V. We are thus brought to the fifth and last book of Dr. Smith's work, in which he examines the important subject of the Public Revenue, or that portion of the revenue of individuals which is allotted to the Expenses of the State. This subject is treated in three subdivisions: the expenses of the commonwealth; the sources of the public revenue; public debts.

* "The member of parliament who supports every proposal for strengthening their monopoly, is sure to acquire not only the reputation of understanding trade but great popularity. If he opposes them, on the contrary, and still more if he has authority enough to be able to thwart them, neither the most acknowledged probity or the highest rank, nor the greatest public services, can protect him from the most infamous abuse and detraction, from personal insults, nay, sometimes from real danger from the insolent outrage of furious and disappointed monopolists." (II. 206.)—"Our great master-manufacturers are as intent to keep down the wages of their own weavers, or the earnings of the poor spinners, and it is by no means for the benefit of the workman that they endeavour either to raise the price of the complete work, or to lower that of the ruder material. It is the industry which is carried on for the benefit of the rich and powerful that is principally encouraged by our mercantile system,—that which is carried on for the benefit of the poor and the indigent is too often either neglected or oppressed." (II. 489.)

i. The expenses of the commonwealth are—*first*, those of defence; *secondly*, those of justice; *thirdly*, those of public works and institutions; *fourthly*, those for supporting the sovereign's dignity.

1. In treating of defence, we are led to consider the progress of the military art. At first, all the clan are warriors, and the chief is the first warrior. In the hunting state, very small bodies can be collected; in the pastoral state only, large bodies may be gathered together; in the infancy of the agricultural state, also, large forces may be raised. But as society advances, manufactures are introduced, and the ruder art of war is improved. It thus becomes doubly necessary to have a certain class of the community trained to arms, and alone called out to serve; for without this, manufacturing industry could not go on, and the military art could not be learnt. If this plan be pursued, a regular army is raised; if the whole citizens in rotation are called upon to serve, it is a militia. The superior efficiency of standing armies has been felt in all ages. Philip of Macedon by their help conquered Greece, and his son conquered Persia. The victories of Hannibal, and, after the second Punic War, those of Rome, were owing to the same superiority. The history of modern wars reads the same lesson. The expense, however, of this mode of defence, now become necessary, is very great in all countries.

2. In early times, the administration of justice in the hands of the sovereign, or of his delegate, was not an expense, but a source of revenue; and hence the greatest abuses, the most sordid corruption, the most cruel injustice, disfigured the administration. Afterwards, justice was said to be administered gratis, that is, by persons whom the sovereign paid; but in all countries fees were exacted from the suitors. Dr. Smith is very far from perceiving the evils of taxing law proceedings; and, indeed, this is one of the parts of his work in which he seems to have taken the least

pains, either to inform himself, or to acquire sound notions of principle. Mr. Bentham has, in his admirable tract on the subject ('Protest against Law Taxes'), demonstrated unanswerably that these imposts are the very worst that have ever, to any considerable extent, been adopted by any civilized nation. Dr. Smith, however, had very sound ideas on the necessity of separating the judicial from the executive office in every State.

3. Institutions or works are of three classes—those for aiding the commerce of the country, those for the education of youth, and those for instructing its adult citizens.

(1.) Those for aiding commerce may either be directed to help the general commerce of a country, or to help particular branches. To the former class belong canals, roads, bridges—of which the cost, either as to making or repairing, may be well and justly defrayed by a toll on those who use them. In some countries, as in France, this expense is defrayed by the State on all the common roads; in others, as in England, the property of tolls is in private hands, and the burthen of repairing the roads lies on them. The repair of the Languedoc Canal was intrusted, with its tolls, to the Engineer Riqueti's family. A local administration in such cases is always better than a central—less costly, and less liable to abuse. To the class of works required for particular branches of commerce belong—factories, established in countries either wholly barbarous, or varying widely in their customs and laws from our own; establishments of Consuls and Ministers; regulated companies, and joint stock companies. Those joint stock companies the members of which have the privilege of transferring their shares, and of being only liable each to the extent of his subscription, have a tendency to draw more capital into the trade than could be invested by the members of private partnerships. Hence they are only to be approved in

cases where there is great public benefit to be derived from the trade they undertake, and where private adventure would be insufficient to conduct it. There seem to be only four kinds of business which justify their formation—banking, insurances, canals, water-works. Had Dr. Smith lived to our day, he would have included railways. The numbers of such companies for purposes of foreign trade which have failed, when not supported by the grant of exclusive privileges, is so great, that, a century ago, the Abbé Morellet enumerated no less than fifty-five such instances in one hundred and fifty years.

(2.) Institutions for the education of children or youth do not necessarily fall on the State to maintain them; they may defray their own expenses. The general rule of such establishments is, that they are founded or endowed by private munificence, sometimes by the bounty of former sovereigns. Dr. Smith contends that their instruction is always worse than that of schools and colleges which subsist by the exertions of teachers paid by school fees. He also objects to such endowments, as drawing to literary pursuits a greater number of persons than would naturally devote themselves to a literary life, or than its gains can support. He seems to admit, however, that there is an advantage even in the small amount of education bestowed in endowed schools and colleges, so very much underrated by him; for he suggests that without them there might have been nothing taught at all. He has even carried his view further, and allowed that the public should establish parish schools; apparently on the ground that the very ignorance which such establishments are calculated to remove, if left to operate, would prevent the bulk of mankind from making any exertion to obtain schools and teachers, by preventing men from being aware of their own deficiencies.

(3.) The institutions for adult education are chiefly those for teaching religion. Dr. Smith does not give a

very decided opinion against an establishment supported by law and by the State, but all, or nearly all his reasoning tends towards that negative; and he gets the better of Mr. Hume's argument, (which he cites as that of "by far the greatest philosopher and historian of the present age,") that there is no better way of preventing the dangers of fanaticism than paying a clergy to be quiet,* by stating that this mischief may be counteracted in two ways: encouraging the study of science not by foundations, but by requiring certain qualifications in philosophical knowledge as the title to offices; and encouraging the arts and amusements, including dramatic exhibitions by which he sets great store. In discussing establishments he touches but slightly on tithes, which he regards as a tax upon the landlord, overlooking the consideration that they are a property which never belonged to him, and are by many reasoners held to be, I think on very doubtful grounds, no more a tax than a rent-charge on his land is. He afterwards recurs to the subject, but no where enters fully into it.

(4.) The expense of maintaining the sovereign's dignity necessarily increases with the progress of luxury and refinement: when all ranks live expensively, the sovereign must be maintained in greater and more expensive luxury than any.

ii. Having considered the expenses which fall upon the government in performing its functions and discharging its duties, we come next to examine the sources from which the funds are derived, to meet those expenses. These funds are of two descriptions; funds belonging to the Sovereign or the State, the revenue of which forms a public income—or income levied from the subjects of the State in the form of taxes. This division of the subject, therefore, is subdivided into two parts.

* "Qui otium reipublicæ perturbant, reddam otiosos." (Cic)

Part 1. The Sovereign or the State may be possessed of property, and frequently has been, of various kinds. It may even have labourers, and employ them at a profit; or it may carry on profitable business on its own account and as a source of revenue. In rude States the Prince profits by the herds which belong to him, and support his expenditure and his power. Where slavery is allowed, the Prince may make a profit by the labour of his slaves. Small republics have driven traffic by their own mercantile profit in various ways. Hamburgh used to have the profit of selling wines in a public wine-cellar, and drugs in an apothecary's shop. Banking was always a source of revenue to the smaller Italian republics, and to Venice, Hamburgh, and Amsterdam. Many Princes have traded like private individuals. The Egyptian Pacha does so at this day; nor is there anything more unfair than such dignitaries entering into competition with their subjects, over whose dealings they exercise a controul. The post-office has always been to the Government of England and other countries a considerable source of revenue. Some Italian and German States have profited by insurance against fire and sea risk. Many of these small States have gained profit by lending at interest their savings or treasure, and thus dealing like other money-lenders. Most States have driven the gainful and dishonest trade of gambling, by way of lottery. But land has in all instances been held by the State. In former times it formed the bulk of the revenue in all feudal countries, the Sovereign being the greatest feudal lord, and defraying all, or nearly all the expenses of his government by his rents as a landowner, while for his military establishment he had to depend upon the precarious and temporary services of the inferior landowners, the crown vassals. It was when the progress of civilization made such military service inconvenient and even impossible, that regular armies became necessary; these required a greater expenditure than

the crown lands could supply; and other sources of revenue became necessary. The other expenses of the Government were increased in proportion, and hence the total inadequacy of the rents compelled the State to provide for the government in all its branches by the levying of money from the people. This gave rise to the modern System of Taxation.

Part 2. Taxes imposed upon the people of any country, must necessarily fall, either upon the rent of land, the profits of stock, or the wages of labour; and a tax may fall on one or more of these three great branches of the income of the community. Hence the subject divides itself into four heads, as taxes are intended to fall upon rents, profits, wages, or on all indiscriminately,—I say, are intended so to fall, because we shall presently see that the incidence of an impost may be very different from that which its authors intended it should be. But there are four leading principles which apply to all taxes whatever, and which must in considering the merits of any given tax be kept always in view. *First.* All the subjects of a State should be called upon to contribute as nearly as possible in proportion to their several means or incomes. *Secondly.* Each individual should be taxed according to a known and certain, and not an arbitrary rule. *Thirdly.* Every tax should be levied in the time and manner most likely to suit the convenience of the contributors. *Fourthly.* Every tax should be so contrived as to take and to keep out of the people's pockets as little as possible beyond what goes into the coffers of the State. A tax may depart from this last principle in four ways: by requiring too large a number to collect and manage it; or by obstructing the people's industry and so injuring the fund of payment; or by encouraging smuggling and thus increasing the price of commodities, while it ruins by prosecutions; or by subjecting the people to vexatious search and other annoyances, which though not directly money

payments, may yet be reckoned as costing what every one would readily give to avoid the evil. This fourth maxim thus appears to be the most important of the whole. According as any tax does or does not conform itself to these several maxims, it is good or bad.

1. A tax on rent may be imposed either by valuing each district at so much yearly, and taking thence a sum, which shall never afterwards be altered; or by taking so much in proportion to the actual rent in every year, or at stated periods of adjustment, and so making the tax rise or fall with the actual value of landed income. In this country the land-tax, settled in the 4th William and Mary, comes under the first of these classes, and therefore sins against the first of the four maxims, but conforms itself to the other three. The second kind of tax is the *Impôt Foncière* of the French Economists. They contend, that all taxes fall ultimately upon rent, and therefore they argue that they ought to be at once and directly imposed upon it. But though Dr. Smith declines a discussion of the metaphysical reasoning by which they maintain such to be the ultimate incidence of all taxes, he yet *undertakes* to show by a review of the facts and arguments that the just conclusion is otherwise. He gives, however, no such proof; he contents himself with a statement taken from the *Mémoires sur les Droits*, published by the French Government, in what manner the tax upon rent and tithes is secured in many of the principal countries of the Continent. He next considers land-taxes, when taken in proportion to the produce and not to the rent; and he shows clearly enough, that these, though advanced by the farmer, are paid by the landlord. Tithe and other such burthens, falling under this description, are unequal because in different lands and different situations, the produce, and consequently the tax, bears a different proportion to the rent. Taxes on the rent of houses, he clearly shows, must fall indifferently on all the sources of revenue,

rent, profit, and wages, the house itself yielding no revenue, and by its use and wear resembling a consumable commodity. As nothing is a better test of a person's whole expenses than the house he lives in, a house tax is recommended by the first maxim, and it suits well enough with the other three. The ground-rent and not the rent payable for profits of building should be the subject of this tax, because that would not raise house-rent, and it would fall heaviest on the capital and larger houses, which can best afford to pay it. As revenue from houses is received without exerting any labour, and with little care either of superintendence or collection, it is a better subject of taxation than land-rents

2. A tax upon the profits of stock must either fall upon the part of the profits which goes to pay the interest of the stock, or the price paid for the stock, or it must fall on the surplus profit over what the interest amounts to. The former revenue belongs to the owner of the stock, the latter being a compensation, generally a very moderate compensation, for the trouble and risk of employing the stock. He cannot pay this himself, for if he did he must run the risk and take the trouble for inadequate reward. Therefore he lays it upon the price of his goods if a trader, or deducts it from the rent if a farmer, or he must take it from the interest, if he does not either raise his prices or lower his rent. Now the interest, though it seems to be, like rent, a fit subject of taxation, is really not so, for two reasons: it is impossible to get at profits of trade as you do at rent, and it is easy to remove stock in trade, while land is not removable. The result has been, that where attempts have been made to tax profits, the State has had recourse to some vague and inaccurate estimate, and has been always content with a very moderate proportion, answering to a very low valuation. Thus our land-tax, though intended to tax all profits, falls mainly on the country and on houses in the towns.

In Holland and in Hamburgh, where stock was taxed, the inhabitants were allowed to assess themselves that an inquisition might be avoided. Had Dr. Smith lived to our days, he would have found some reason to be confirmed in his opinion of the land paying far more than its share, owing to its being irremovable and unconcealable; but he would also have seen how considerable an approximation to equal payment could be made by inquisitorial proceedings, and well-constructed machinery.—Taxes laid on particular trades must fall on the consumer, as the dealer will not remain in a business which does not yield the average rate of profit. A tax on all profits of one trade, but proportioned to each dealer's trade, finally falls on the consumer; if not so proportioned it falls on the consumer, but favours great and oppresses small dealers. The shop-tax once proposed had this disadvantage in a great degree; for all shops must have paid. The personal *taille* in France was a tax upon farmers' profits, and as a farmer paying rent never can withhold his crop from the market in order to raise his prices, he can only throw the taille on the landlord by lowering his rent. The tax being levied according to the farmer's stock, made every one stock his farm as badly as possible, and endeavour to conceal the stock he had. Poll-taxes in countries having slaves, are taxes on profits. Poll-taxes on free men are of a wholly different nature, and are the most unequal of all. Taxes on household servants are taxes on consumption, and they are objectionable because servants are not employed in proportion to the income of their masters; then these taxes fall heavier on the middle classes, and not at all on the lower orders, unless so far as they may prevent some from finding employment.

An Appendix on this head discusses Taxes on Capital, which have not generally been intended to be levied by any State; all the imposts of this kind being meant to affect income only. But when property

changes hands by death, then both the Romans in Augustus' time, the Dutch, the English, and all feudal countries, in taxing the casualties, intentionally levied imposts upon capital. The feudal perquisites on alienation operated when property was sold. Stamp duties on purchases have with us the same operation. Taxes on succession fall on the owner; taxes on sale fall on the seller, because he is the needy person and must pay. The Spanish *Alcavala* seems to be of this class, though Dr. Smith does not here consider it. All taxes on capital are unthrifty, because they diminish the fund for employing labour and machinery, or increasing production. Living upon the principal, is accordingly a common expression to denote the usual spendthrift course.

It must be observed that Dr. Smith in this, as in other parts of his work, leaves out of view one important circumstance when speaking of capitalists, and also of labourers, shifting their stock or their labour to new channels of employment when a burthen is laid on them, or any other demand is made which tends to lower their gains. They very often linger on a long time, perhaps all their lives, in order to avoid the disagreeable consequences of the change; and because they have become expert in one employment and could not soon be equally so in another. What they would pay to avoid a risk or a disagreeable change of employment or business, may fairly be reckoned the difference of the two in value to them, according to an argument often used by Dr. Smith, and this price they pay for continuing in their former business or occupation. It is also to be observed that Dr. Smith, when he speaks of the tax often being thrown on the consumer, forgets the important consideration that the power of so throwing it depends on the condition of the market. When the demand is rising, or even stationary if steady, the tax may be thrown on the consumer; when the market is falling, or is fluctuating,

the trader is unable so to throw it, and he must either pay it himself or quit the trade.

3. Taxes on wages must be paid by the rise of wages a good deal higher than the tax; the tax is not even advanced in the first instance by the labourer, but by his employer, who must lay it on goods, or deduct it, if a farmer, from rent. Hence the consumer or the landlord must always pay such taxes. The French taille was charged on labourers as well as farmers, and produced great evils. In Bohemia artificers paid a tax of ten pounds a year in the highest class, and so down to two pounds ten shillings in the lowest. The emoluments of office-bearers if so taxed do not fall under the same rule, as the competition is not open. The tax on these falls on the officer.

4. The taxes intended to fall on all the three, funds, rent, profits and wages, indiscriminately, are capitation taxes, and those on consumable commodities.

(1.) Poll-taxes are utterly unjust if they be not apportioned to fortune; even then a great injustice must take place, and a yearly inquisition is necessary, as a man's fortune is constantly varying. If they are, as our poll-tax of William III.'s time, laid on rank, they are manifestly unequal. In France the poll-tax was laid on the higher orders by a tariff according to rank; on the lower and middle classes it was levied according to property, and subjected the people to a severe inquisition. In so far as the taxes fall on the lower orders they are levied on wages, and liable to the objections stated to those imposts. The difficulties of a poll-tax being applied to expenditure or income gave rise to the taxes on consumable commodities.

(2.) These commodities are either necessaries or luxuries. Taxes on the former would be perfectly unequal if their incidence was ultimately what it is intended to be in the first instance; but they are really taxes on labour, and must fall on the employer, not on the workman, the employer laying them on the land-

lord or the consumer. Those on luxuries are not so transferred, even those on the luxuries of the poor. Thus the duties on beer and tobacco do not raise wages, nor materially diminish the power of bringing up a family; nor do they necessarily raise the price of any except the taxed commodities. The taxes on the four necessaries, salt, leather, soap and candles, affect in some small degree the wages of labour; however, the salt-tax, now repealed (somewhat hastily, by the efforts of party,) pressed so very lightly that its loss has been pretty generally lamented, and it certainly yielded to the clamour against its disproportion to the price of the article, and its requiring so many persons to collect it. Dr. Smith, however, condemns much more strongly two other measures which operate as taxes on the mere necessaries of life, and yield no revenue; the bounty on exportation of corn, and the protecting duties on the importation of that and meat. But he considers these as clearly tending to raise the price of labour, and consequently regards their repeal as sure to lower wages; so that the advocates of that repeal are prevented from quoting his authority because they always deny this tendency of the measure, or at least have always denied it since the working-classes hearing the arguments originally advanced for the repeal, from its being expected to lower wages, plainly indicated their aversion to the change. Dr. Smith shows that in other countries a high direct tax is imposed on flour, and even on bread, instancing Holland, where it was supposed to make the money price of bread double in the towns; the country inhabitants paying a poll-tax in lieu of it. The taxes on luxuries fall pretty equally on the whole people, according to their consumption. The great bulk of them is paid by the inferior and most numerous classes, but no rise of wages being caused by this payment, the burthen remains where it first falls. Dr. Smith strongly recommends the repeal of beer-taxes, and substituting malt-taxes instead; this has since been

so far effected that beer is no longer directly taxed. But these taxes especially, on the upper classes, do not fall in proportion to income, for they are proportioned to expenditure only, which varies much more in the higher classes than in the middle and lower ranks. Absentees, too, pay no such taxes, and accordingly Dr. Smith is an advocate for absentee taxes, giving Ireland as an example of the effects of persons being non-resident on their estates, and wholly forgetting that an Irish family residing in England contributes to the revenue by which Ireland is governed and defended, as much as a Scotch family living in London does to the government and defence of Scotland; or a Yorkshire family to that of Yorkshire. He shows, however, very clearly that all taxes upon consumable commodities sin against the fourth maxim; they keep and take more from the people than almost any others, creating a number of excise and customs officers, by raising prices and discouraging consumption, by vexatious prosecutions for smuggling, and by vexatious visits of officers. He here discusses the *alcavala*, or tax on sales of all kinds, in Spain, at first of ten and even fourteen per cent., and afterwards of six per cent., and a similar tax of three per cent. on all contracts in the Spanish kingdom of Naples. He institutes an interesting comparison between the old system of taxation in France, and that of England, giving the clear advantage to the latter.

Upon the whole it must be admitted, that the long chapter on taxation, (one of the longest, having 153 pages), though from the variety of the facts brought together, it is exceedingly entertaining, is less instructive than any other part of the 'Wealth of Nations;' because the principles are not very fully and carefully discussed, because the whole operation of the different taxes described is not accurately traced, and because, therefore, the important point of their ultimate incidence is not accurately and satisfactorily pursued and

explained. Some of the most important taxes are very slightly touched upon, and the subject of an income-tax is very imperfectly handled. The doctrine of the Economists of a single tax, *impôt foncière*, being substituted for all others, is rather indirectly treated than fully and authoritatively exposed, while so great an error claimed ample refutation; and the manifest fairness as well as advantage of so distributing taxes, as to give every variety to them, and thus to make their ultimate incidence as universal as possible, and yet as far as possible proportionate to the means of payment, is not at all dwelt upon, hardly touched.

iii. In the early stages of society and of government, the Sovereign always making provision for extraordinary occurrences, used to amass out of his annual income, either accruing from property or obtained by taxes, savings which formed a treasure in course of time. Even as far down as the early part of the eighteenth century, the Prussian treasure enabled Frederic II. to carry on successful wars almost as much as the disciplined army, to which he succeeded from his father. But in our times extraordinary emergencies are met by borrowing; and all Governments are more or less in debt, many of them heavily indebted. It is much easier for the Government of a commercial country to raise loans than for any other, because capitalists are ever to be found able and willing to advance money on the public security. For the most part these loans have at first been personal, that is, on the general credit of the Government; afterwards when that was exhausted, the lenders required security, and branches of the public revenue were mortgaged for repayment of the loans. The unfunded debt of this country belongs to the former class, the funded to the latter. The convenience of raising supplies by loan is obvious; but its mischievous consequences are as manifest, and they very far counterbalance its advantages. Were all supplies required

for a war to be raised by taxes within the year, or were this the general rule, then would the reluctance to engage in war, and the readiness to make peace after the war had been begun, be incalculably increased and universally diffused; and a loan might always be resorted to as an exception to the rule when public feelings were directed against continuing a war absolutely necessary for the honour, that is, for the existence of the State. These I place as synonymous ideas, because no war, however short, can ever be beneficial on a calculation of profit and loss; and thus only those wars are justifiable on sound policy which are required by the necessity of averting national disgrace, and are entered into for the national independence, placed in imminent peril by submitting to insult, as a man's whole fortune is by consenting to pay money under a threat, or submitting to any other extortion. But for this consideration no one would defend an action, or sue a debtor for a small sum of money, even if his adversary admitted himself to be in the wrong.

The payment, or the escape from the payment of debts, forms an important subject of consideration in this discussion. Generally speaking, the latter course has been taken when the burthen became heavy. The most common expedient, the most hurtful, and the most disgraceful, has been tampering with the coin. This has been done in two ways,—one by raising its denomination, making, for instance, every pound be called two pounds; the other, by debasing it with alloy: and these two expedients differ only in the form,—the one being an act of open violence, the other an act of secret fraud; but both have the effect of cheating all creditors, not only those of the State, but those of private debtors, to the amount of the difference between the two nominal values in the one case, and the two real values in the other. Most countries have had recourse to one or both of these expedients, and it is

of ancient origin; for the Romans had first by one and then by the other expedient, before the end of the second Punic war, made the coin worth nominally two-and-twenty times more than it originally was.

Incited by a view of the dangers of taxation, perpetuated by public debts, Dr. Smith strongly recommends the increase of such taxes as are most according to principle, and fall in with the four general maxims already stated; but above all, he recommends in what he admits to be a kind of "New Utopia," but not more useless and chimerical than the "old one," a general union of the whole empire, by giving both Ireland and all the colonies representatives, and thus making all parts of our dominion contribute to a fund for paying off the debt which was contracted for the government, and the defence of them all. This plan with its details, closes the work. The recommendation as regards Ireland has been successfully adopted and carried into execution. It was soon made clear by the events of the American war that no such incorporation of the distant provinces could be effected. Mr. Burke, in a speech on conciliation with America, adverted to such a plan and said, "A great flood stops me in my course. *Opposuit natura.* I cannot remove the eternal barriers of the creation."* No representative Government ever can be maintained, when the delegate and his constituents live on the opposite shores of the Atlantic.

Having now finished the analytical view of this great work, the opinion may, in conclusion, be expressed, which all men are now agreed in entertaining of its prodigious merits. It may truly be said to have founded the science of Political Economy, as it exists in its new and greatly improved form. Many preceding authors had treated different branches of the sub-

* Works, iii. 91.

ject; some, as we have seen in the introduction to this Life, had, before Dr. Smith's time, treated several of those branches upon the sound and rational principles which he applied to economical questions. Systematic treatises were not wanting which professed to embrace the whole as a science; and of these the most extensive and most valuable was Sir James Stewart's. But the 'Wealth of Nations' combines both the sound and enlightened views which had distinguished the detached pieces of the French and Italian Economists, and above all, of Mr. Hume, with the great merit of embracing the whole subject, thus bringing the general scope of the principles into view, illustrating all the parts of the inquiry by their combined relations, and confirming their soundness in each instance by their application to the others. The copiousness of the illustrations keeps pace with the closeness of the reasoning; and wherever the received prejudices of lawgivers are to be overcome, or popular errors to be encountered, the arguments, and the facts, and the explanations are judiciously given with extraordinary fulness, the author wisely disregarding all imputations of prolixity or repetition, in pursuit of the great end of making himself understood, and gaining the victory over error. The chapter on the Mercantile System is an example of this; but the errors of that widely prevailing theory and its deeply-rooted prejudices are also encountered occasionally in almost every other part of the work.

It is a lesser, but a very important merit, that the style of the writing is truly admirable. There is not a book of better English to be any where found. The language is simple, clear, often homely like the illustrations, not seldom idiomatic, always perfectly adapted to the subject handled. Beside its other perfections, it is one of the most entertaining of books. There is no laying it down after you begin to read. You are drawn on from page to page by the strong current of

the arguments, the manly sense of the remarks, the fulness and force of the illustrations, the thickly strewed and happily selected facts. Nor can it ever escape observation, that the facts, far from being a mere bede-roll of details unconnected with principle and with each other, derive all their interest from forming parts of a whole, and reflecting the general views which they are intended to exemplify or to support.

This admirable work has received the aid of several learned and able commentators, of whom Professor Macculloch is, beyond all question, the first in this country, and M. de Garnier abroad. The edition of the former is a book of great value, and like his excellent treatise on Political Economy in the 'Encyclopædia Britannica,' ought to be in the hands of every one who would study this science with success.*

APPENDIX.

I. Economists and Dr. Smith.

The two leading opinions which divide political inquirers upon the sources of national wealth, are those of the Economists and of Dr. Smith. We purpose here to exhibit a concise view of the objections to which both of these doctrines are eminently liable. As the general principle of a distinction between productive and unproductive labour is recognized by Dr. Smith,—as we conceive his theory to be extremely inconsistent with itself, and consider it to be an imperfect approximation to that of the Economists, we shall begin with a short examination of the principle on which it depends. That eminent writer divides labourers into two classes: those who, by adding to the value of some raw material, or by assisting in the increase of their quantity, realize or fix in a vendible commodity the effects of their

* The editions of Dr. Smith's works referred to in this Life are, 'Moral Sentiments,' London, 1792, and 'Wealth of Nations,' London, 1802; being the seventh of the former, and the tenth of the latter.

exertions; and those whose labour leaves nothing in existence after the moment of exertion, but perishes in the act of performance. The former he denominates *productive*, the latter *unproductive* labourers; not meaning thereby to undervalue the exertions of many useful kinds of work performed by the unproductive order, but merely asserting that they do not augment the *wealth* of the community. Thus, the work of the farm-servant, or manufacturing labourer, is fixed in a useful commodity; the work of a menial servant perishes with the motion of his hands, and adds to the value of nothing. A man grows rich by employing a number of the former; he ruins himself by keeping a multitude of the latter.

To begin with this illustration. The case of the menial servant must not be compared with that of the labourer employed in farming or manufactures. The menial is employed by the *consumer*, and for his own use exclusively; the farm-servant and journeyman are employed by another party, by whom the consumer is supplied. The former is, properly speaking, in the predicament of a commodity bought or hired for consumption or use; the latter rather resembles a tool bought or hired for working withal. But, at any rate, there is no such difference as Dr. Smith supposes between the effects of maintaining a multitude of these several kinds of workmen. It is the extravagant quantity, not the peculiar quality of the labour thus paid for, that brings on ruin. A man is ruined if he keeps more servants than he can afford or employ, and does not let them out for hire,—exactly as he is ruined by purchasing more food than he can consume, or by employing more workmen in any branch of manufactures than his business requires, or his profits will pay.

But it may be observed, in general, that there is no solid distinction between the effective powers of the two classes whom Dr. Smith denominates productive and unproductive labourers. The end of all labour is to augment the wealth of the community; that is to say, the fund from which the members of that community derive their subsistence, their comforts and enjoyments. To confine the definition of wealth to mere subsistence is absurd. Those who argue thus admit butcher's meat and manufactured liquors to be

subsistence; yet neither of them are necessary; for if all comfort and enjoyment be kept out of view, vegetables and water would suffice for the support of life; and by this mode of reasoning the epithet of *productive* would be limited to the sort of employment that raises the species of food which each climate and soil is fitted to yield in greatest abundance, with the least labour;—to the culture of maize in some countries; of rice in others; of potatoes, or yams, or the bread-fruit tree in others: and in no country would any *variation* of employment whatever be consistent with the definition. According to this view of the question, therefore, the menial servant, the judge, the soldier and the buffoon, are to be ranked in the same class with the husbandman and manfacturers of every civilised community. The produce of the labour is, in all these cases, calculated to supply either the necessities, the comforts, or the luxuries of society; and that nation has more real wealth than another, which possesses more of *all* those commodities. If this is not admitted, then we can compare the two countries only in respect of their relative shares of articles indispensably requisite, and produced in greatest abundance, considering the soil and climate of each: and, as nothing which is not necessary is to be reckoned valuable, a nation wallowing in all manner of comforts and enjoyments is to be deemed no richer than a horde fed upon the smallest portion of the cheapest grain, or roots and water, which is sufficient to support human life.

But it is maintained that, admitting the wealth of a community to be augmented by the labour of those whom Dr. Smith denominates unproductive, still they are in a different predicament from the productive class, inasmuch as they do not augment the exchangeable value of any separate portions of the society's stock—neither increasing the quantity of that stock, nor adding to the value of what formerly existed. To this, however, it may be replied that it appears of very little consequence whether the wants of the community are supplied directly by men, or mediately by men with the intervention of matter—whether we receive certain benefits and conveniences from those men at once, or only in the form of inanimate and disposable substances. Dr. Smith would admit that labour to be productive which

realized itself in a stock, though that stock were destined to perish the next instant. If a player or musician, instead of charming our ears, were to produce something which, when applied to our senses, would give us pleasure for a single moment of time, their labour would be called productive, although the produce were to perish in the very act of employment. Wherein, then, lies the difference? Merely in this—that we must consume the one produce at a certain time and place, and may use the other in a latitude somewhat, though but little, more extensive. This difference, however, disappears altogether, when we reflect that the labour would still be reckoned productive which should give us a tangible equivalent, though it could not be carried from the spot of its production, and could last only a second in our hands upon that spot. The musician, in reality, affects our senses by modulating the air; *i. e.*, he works upon the air, and renders a certain portion of it worth more than it was before he manufactured it. He communicates this value to it only for a moment, and in one place; there and then we are obliged to consume it. A glassblower, again, prepares some metal for our amusement or instruction, and blows it up to a great volume. He has now fixed his labour to a tangible commodity. He then exchanges it, or gives it to us, that we may immediately use it; *i. e.*, blow it until it flies to shivers. He has fixed his labour, however, we say, in a vendible commodity. But we may desire his farther assistance—we may require him to use it for our benefit; and, without any pause in his process of blowing, he bursts it. This case approaches as nearly as possible to that of the musician; yet Dr. Smith maintains that the latter is a different kind of labour from the former. Nay, according to him, the labour of the glassblower is productive, if he spoils the process, and defeats the end of the experiment, by pausing, and giving into unskilful hands the bubble before it bursts. But if he performs the whole of that instructive operation, by contemplating which Sir Isaac Newton was taught the nature of colour, his labour must be denominated unproductive!

But it is not fair to deny that the class called unproductive fixes its labour in some existing commodity. First, we may observe that no labour, not even that of the far-

mer, can lay claim to the quality of actually *adding* to the stock already in existence: man never creates; he only modifies the mass of matter previously in his possession. But, next, the class alluded to does actually, like the class termed unproductive, realize its labour in an additional value conferred upon the stock formerly existing. The only difference is, that instead of working upon detached portions, this class operates upon the stock of the community in general. Thus, the soldier renders every portion of the stock more valuable by securing the whole from plunder; and the judge, by securing the whole from injury. Dr. Smith would allow that man to be a productive labourer who should manufacture bolts and bars for the defence of property. Is not he also, then, a productive labourer, who protects property in the mass, and adds to every portion of it the quality of being secure? In like manner, those who increase the enjoyments of society, add a value to the stock previously existing; they furnish new equivalents for which it may be exchanged; they render the stock worth more, *i.e.*, exchangeable for more—capable of commanding more enjoyments than it formerly could command. The stock of the community is either that part which is consumed by the producer, or that part which he exchanges for some object of desire. Were there nothing for which to exchange the latter portion, it would soon cease to be produced. Hence the labour that augments the sum of the enjoyments and objects of desire for which this portion may be exchanged, is indirectly beneficial to production. But if this portion destined to be exchanged, is already in existence, the labour which is supported by it, and which returns an equivalent to the former owner by the new enjoyments that it yields him, must be allowed to add a value directly to the exchangeable part of the stock.

It appears peculiarly inconsistent in Dr. Smith to deny that labour can add to value by its general operation on the stock of the community, and on the fund of equivalents, when we find him frequently reckoning things by other than physical means, measuring them by other standards than actual bulk and quantity—nay, counting their price in money when no money can be exchanged for them. He approaches often nearer than any assignable distance to the

doctrines which I have been explaining. Thus he more than once, but particularly in the inquiry concerning taxation (Book vi. chap. 2), when mentioning the trouble or annoyance which certain things occasion, says they may be estimated at the sum any one would willingly give to be rid of them, and he considers the impost which is levied by means so vexatious as increased in its amount by that sum. Why not consider the sum also which any one would give to secure his property from the risk of an invasion, or of pillage in a riot, as increasing the value of that property? Now the obtaining this security is the service which Government renders to the owner of the property by defence and police; it is the service for which their wages are paid to soldiers, and magistrates, and police officers. Can we then, on Dr. Smith's own view, deny the additions made to the stock of the community by these labourers, or refuse to their labour the name of productive?

In every point of view, therefore, it appears that the opinion of Dr. Smith is untenable. He has drawn his line of distinction between productive and unproductive labour in too low a part of the scale. The labour which he denominates unproductive has the very same qualities with a great part of the labour which he allows to be productive. According to his own principles, the line should have been drawn so as to cut off, on the one hand, the labour which apparently increases the quantity of stock, and to leave, on the other hand, all that labour which only modifies, or in some manner induces a beneficial change upon stock already in existence. In a word, his principles clearly carry him to the theory of the Economists; and, in order to be consistent, he ought unquestionably to have reckoned agriculture the *only* productive employment of capital or labour. That there is only this one doctrine tenable, in consistency with itself, has been, we conceive, sufficiently proved. We shall now consider whether there is in reality any foundation even for this distinction, which forms the basis of the theory supported by the Economists.

Whoever has honoured the foregoing observations with his attention, will speedily be satisfied that the reasonings applied to Dr. Smith's classification of labour are applicable also to the more precise and constituent doctrine of the

followers of Quesnay. It is the opinion of these ingenious metaphysicians that the labour bestowed upon the earth can alone be considered as really productive; that all other labour only varies the position or the form of capital, but that agriculture increases its net amount. That the merchant who transports goods from the spot of their abundance to the quarter where they are wanted adds nothing to the whole stock or to the value of the portions which he circulates, these reasoners deem almost a self-evident proposition. That the manufacturer who fashions raw materials into useful commodities increases their value, the Economists indeed admit, but they deny that any further addition is thus made to the value of the materials than the value of the workman's maintenance while employed in the manufacture.

It seems obvious, at first sight, to remark, that, according to their own principles, these theorists have committed one error. They have ranged all labour, except that of the husbandman, in the same class; while they have virtually acknowledged that as great a difference subsists between the two members of that division, as between either of them and the other division. For surely, the merchant, who adds, according to them, no value to any material, is as much to be distinguished from the manufacturer who does add the value of his maintenance to the raw produce, as the manufacturer is to be distinguished from the husbandman, whose labour returns a net profit over and above the price of his maintenance. This criticism is almost decisive, in a discussion which, it must be admitted on all hands, resolves into a question of classification. But the error of the Economists is still more fundamental.

There is no essential difference between the powers of man over matter, in agriculture, and in other employments. It is a vulgar error to suppose that, in the operations of husbandry, any portion is added to the stock of matter formerly in existence. The farmer works up the raw material, *i. e.*, the manure, soil, and seed, into grain, by means of heat, moisture, and the vegetative powers of nature, in whatever these may consist. The manufacturer works up his raw material by means of certain other powers of nature. Dr. Smith, however, who states the doctrine of the Econo-

mists in its greatest latitude, (Chap. V., Book II., Vol. II., p. 52, 8vo. edition), asserts, that, in agriculture, nature works with man, and that the rent is the wages of her labour; but that, in manufactures, man does everything. But does not nature work with man in manufactures as well as in agriculture? If she works with him in forming a handful of seed into a sheaf of flax, does she not also work with him in fashioning this useless sheaf into a garment? Why draw a line between the two effects, when a person can no more clothe himself with an unwrought sheaf of the produce than with an unsown handful of the seed. Why draw a line between the two operations, when the workman can no more change the sheaf into a garment without the aid of those powers which we denominate nature, cohesion, divisibility, heat, and moisture, than the farmer can convert the seed into a sheaf, without the vegetative powers of heat, moisture, and cohesion? If, instead of flax, we suppose the sheaf to be of barley, the analogy will be still more apparent. The brewer or distiller is certainly a productive labourer; yet the changes which he effects are as little the direct work of his hands, as the multiplication of the seed in the field. The conversion of that substance into an intoxicating beverage, is the work of nature, as well as its growth in the harvest; and fermentation is as great a mystery as vegetation. If the rent of land, again, may be called the wages of nature, in agricultural operations, the net profits of manufacturing stock may be termed her wages in our operations upon raw produce; meaning, by net profits, that part of the gross profit which remains after paying the labourer who works, and him who superintends; that is, after deducting wages, and the profit received by a man trading on borrowed capital; for we must always keep in view a consideration, the omission of which we will venture to assert, has misled almost all political inquirers, that the rent of land is, properly speaking, the net profit of stock advanced by the landlord, and that everything which the farmer receives, over and above the wages of his labour, is the profit of another stock, which may be borrowed as well as the land; and in this case his whole profit resolves into wages—the case of a trader having no capital whatever. In both cases there is a clear gain; in both it is obtained

in the same way; in both distributed among the same classes.

Let us, however, take an example or two, for the purpose of comparing more closely the productive with the unproductive kinds of labour. The person who makes a plough is, according to the Economists, an unproductive labourer, but he who drives it is a productive labourer. In what predicament, then, is the labourer who makes a hedge round a field for its protection, or a ditch for draining it? This operation, because it is called farm-work, is admitted by the Economists to be productive. But wherein does it differ from the plough manufacture? Both are alike subservient and necessary to the operations of ploughing and reaping; both are alike performed by persons who do not raise the produce that feeds them;—and both are alike performed upon some materials produced from the earth by other labour. If the plough were made in a bungling manner by farm-servants in the out-houses of the farm, we imagine the manufacture would of necessity fall under the head of productive labour, as well as the work of hedging and ditching. Again—Capital employed by the corn-merchant in collecting and circulating grain, is most unproductively employed, according to the Economists. But the capital employed in collecting seed in a barn, carrying it from thence to the field, and returning the crop at harvest, is employed in the most productive manner possible. Can it be maintained that there is any difference whatever between these two cases, necessarily placed by the theory of the Economists at the opposite extremes of their scale? If the corn-merchant lived on the ground of the farmer, and if the farmer, from this convenient circumstance, were enabled to sell all his grain without having any barns or granaries, certain of supplying himself at his own door next seed-time, the Economist would be forced to allow that the capital of the corn merchant, in so far as it assisted the farmer, was productively employed.—Wherein lies the difference?—And these observations are applicable to every case of every manufacture, aud every species of commerce whatever. They apply to those kinds of employment which are subservient to the purposes of comfort and enjoyment, as well as to those which administer to our necessary wants; for

we showed above, that there is no possibility of drawing a line between the cases, consistently with principles admitted even by the Economists themselves. The foundation of all these misapprehensions is evidently laid in a neglect of the great principle of the division of labour. In whatever part of a community the labour connected with agriculture, immediately or remotely, is performed, the subdivision of the task renders it more productive than if it were carried on upon the farm itself; and, to deny the same properties to this labour, on account of its subdivision and accumulation in different quarters, is little less than a contradiction in terms.

There is only one view of the Economical theory which remains to be taken: it is that most ingenious argument by which the followers of Quesnai attempt to prove, that manufacturing labour only adds a value equal to its own maintenance. The above remarks may indeed suffice for the refutation of this doctrine, but its peculiar demonstration merits separate attention.* The works of the artizan, the Economists maintain, are in a very different predicament from the produce of the agricultural labourer. Multiply the former beyond a certain extent, and either a part will remain unsold, or the whole will sell at a reduced price. Multiply the latter to any extent, and still the same demand will exist, from the increased number of consumers, whom it will maintain. The labour of the artisan is therefore limited to a particular quantity; this quantity it will always nearly equal, but never exceed; and the amount is determined by the competition of different artists on the one hand, and the fixed extent of the demand on the other. The labour of the husbandman has no such limits. The extension of his productions necessarily widens his market. The price of manufactures will therefore be reduced to the value of the raw material, of the workman's maintenance, and of his master's maintenance; while that of agricultural produce, having no such limit, leaves always a net profit over and above the farmer's maintenance.

In answer to this very subtle argument, we may remark, that it proceeds on a total misconception of the principle of

* See this reasoning stated repeatedly in *Dialogue 2de*, *Physiocratie*, p. 571.

population. It is absurd to suppose that the mere augmentation of agricultural produce extends the demand for it, by increasing the population of the community. If the lowest means only of subsistence are considered, and if men will be contented to possess only the simplest food, without any raiment, then, no doubt, an increase of grain and roots may increase the numbers of the consumers. But is it not evident that men require more than the mere necessaries of life, and that even those necessaries are in part the production of manufacturing labour? Does not a person in forming his estimate of a competency, take into the account articles of manufacture as well as husbandry, furniture, clothes, and even luxuries—gratifications as well as meat and drink? The mere augmentation of those simple necessaries will never sensibly increase the number of the consumers, any more than the mere augmentation of articles of comfort and luxury. An increase in the production of the one class of commodities will operate exactly as powerfully on population, as an increase in the production of the other class. In fact, an increase of either may somewhat affect the numbers of the consumers; but in order to produce any considerable augmentation of those numbers, the increase of both species of produce must go on together. This argument, then, only leads us by a new, and certainly an unexpected road, to an additional conclusion in favour of the theory that utterly denies all distinction between any of the applications of capital and industry, which are subservient to the wants and enjoyments of man.

The reasoning in which we have been engaged, will probably be deemed sufficient to authorize several positive inferences with respect to the nature and sources of national wealth. We trust that enough has been said to expose the inaccuracy of drawing any line between the different channels in which capital and labour may be employed—of separating, with Dr. Smith and his followers, the operations of agriculture, manufactures, and commerce, from those arts where nothing tangible is produced or exchanged—or of placing, with the Economists, the division somewhat higher, and limiting the denomination of *productive* to agricultural employment alone. It may safely be concluded, that all those occupations which tend to supply the necessary wants, or

to multiply the comforts and pleasures of human life, are equally productive in the strict sense of the word, and tend to augment the mass of human riches, meaning, by riches, all those things which are necessary, or convenient, or delightful to man. The progress of society has been attended with a complete separation of employments originally united. At first, every man provided for his necessities as well as his pleasures, and for *all* his wants as well as *all* his enjoyments. By degrees, a division of those cares was introduced: the subsistence of the community became the province of one class, its comforts of another, and its gratifications of a third. The different operations subservient to the attainment of each of these objects, were then intrusted to different hands; and the universal establishment of barter connected the whole of the divisions and subdivisions together; enabled one man to manufacture for all, without danger of starving by not ploughing or hunting; and another to plough or hunt for all, without the risk of wanting tools and clothes by not manufacturing. It has thus become as impossible to say exactly who feeds, clothes, and entertains the community, as it would be impossible to say which of the many workmen employed in the manufacture of a pin is the actual pin-maker, or which of the farm-servants produces the crop. All the branches of useful industry work together to the common end, as all the parts of each branch co-operate to its particular object. If you say that the farmer feeds the community, and produces all the raw materials which the other classes work upon; we answer, that unless those other classes worked upon the raw materials, and supplied the farmer's necessities, he would be forced to allot part of his labour to this employment, while he forced others to assist in raising the rude produce. In such a complicated system, it is clear that all labour has the same effect, and equally increases the whole mass of wealth. Nor can any attempt be more vain than theirs who would define the particular parts of the machine that produce the motion, which is necessarily the result of the whole powers combined, and depends on each one of the mutually connected members. Yet so wedded have those theorists been to the notion, that certain necessary kinds of employment are absolutely unproductive, that a writer of no less name

than Dr. Smith has not scrupled to rank the capital sunk in the public debt, or spent in warfare, in the same class with the property consumed by fire, and the labour destroyed by pestilence. He ought surely to have reflected, that the debts of a country are always contracted, and its wars entered into, for some purpose either of security or aggrandizement; and that stock thus employed must have produced an equivalent, which cannot be asserted of property or population absolutely destroyed. This equivalent may have been greater or less; that is, the money spent for useful purposes may have been applied with more or less prudence and frugality. Those purposes, too, may have been more or less useful; and a certain degree of waste and extravagance always attends the operations of funding and of war. But this must only be looked upon as an addition to the necessary price at which the benefits in view are to be bought. The food of a country, in like manner, may be used with different degrees of economy; and the necessity of eating may be supplied at more or less cost. So long as the love of war is a necessary evil in human nature, it is absurd to denominate the expenses unproductive that are incurred by defending a country; or, which is the same thing, preventing an invasion, by a judicious attack of an enemy; or, which is also the same thing, avoiding the necessity of war by a prudent system of foreign policy. And he who holds the labour of soldiers and sailors and diplomatic agents to be unproductive, commits precisely the same error as he who should maintain that the labour of the hedger is unproductive, because he only protects, and does not rear the crop. All those kinds of labour and employments of stock, are parts of the system, and all are *equally* productive of wealth.*

* See Book II. chap. III. 'Wealth of Nations.' (Vol. II., page 25, 8vo edition.) The terms *productive* and *unproductive* are, in the argument of some of the Economists, and in parts of Dr. Smith's reasonings, so qualified, as to render the question a dispute about words, or at most about arrangement. But this is not the case with many branches of both these theories, and especially with the position examined in the text. The author actually remarks how much richer England would now be, had she not waged such and such wars. So might we estimate how many more coats we should have, had we always gone naked. The remarks here stated, may with equal justice be applied to a circumstance in the Theory

II. Capital.

By capital, when used generally, we understand the whole of the material world which man can appropriate, as well as those talents, natural or acquired, which are the springs of his exertions. In this sense of the word, it signifies all property material and mental, or every thing valuable to man. Among other things, it clearly comprehends land. But sometimes we speak of capital, in opposition to land; and, in this case, it comprehends every thing valuable, except the ground; for it certainly includes all the parts and productions of the soil which are severed from it. In this sense, the division nearly resembles the legal distribution of property into real and personal. Both these definitions of capital are used repeatedly, and with equal frequency, by every writer on political economy.

If capital is contradistinguished from land, the separation is made by a most indefinite and obscure boundary. Canals, roads and bridges, are as much a part of capital, as any portable machines, fashioned out of the produce or parts of the soil. The same may be said of fences, drains, footways, and in general of all the ostensible monuments of labour in an improved farm. But is not the soil itself, also, referable to the very same class, after it has been worked up with manure and composts, so as to be highly fertilized? Is not the whole surface of an improved farm, therefore, to be considered as capital, rather than as land? And when a person buys a hundred acres of improved land, how can he say what part of the price is paid for land, and what part for capital? We speak indeed of capital vested in land, and use the phrase, until we actually think there is such a thing as

of the Balance of Trade. In stating the proportion of exports to imports, it has justly been observed, that no notice can ever be taken, in Customhouse accounts, of money remitted for subsidies, or for the payment of our troops and fleets abroad. But it has very inaccurately been added, that these sums are so much actually sent out of the country without an equivalent. In fact, the equivalent is great and obvious, although of a nature which cannot be stated in figures among the imports. The equivalent is all the success gained by our foreign warfare and foreign policy—the aggrandisement and security of the State, and the power of carrying on that commerce, without which there would be neither exports nor imports to calculate and compare.

adding the capital to land; whereas the whole meaning of the expression is, that capital of one kind or other is given in exchange for land, or that our property has become land, instead of some other valuable commodity—or, according to what has just now been defined, that one kind of capital has been exchanged for another. If it is said, that capital is that in which labour has been fixed and realized, either by accumulation or by change of form; then, it is very obvious, that land, in the most extensive sense of the word, must become capital in order to be useful; and that many things, usually reckoned capital, as the wild produce which is raised by nature without human assistance, belongs to the class of land, and not to that of stock. But a difference is established by some, especially by Dr. Smith, between capital and the other parts of stock; capital being, according to them, that part which brings in a revenue. This idea clearly appears, by the whole of the illustrations given of it, to have arisen from the fundamental error of considering nothing as productive which does not yield a tangible return, and of confounding use with exchange. For, may not a man live upon his stock, that is, enjoy his capital, without either diminishing or exchanging any part of it? In what does the value, and the real nature of stock reserved for immediate consumption, differ from stock that yields what Dr. Smith calls a revenue or profit? Merely in this—that the former is wanted and used itself by the owner; the latter is not wanted by him, and therefore is exchanged for something which he does want. There is surely no other meaning in the idea of profit or revenue, but this: and as the profit of that part of stock which is exchanged, and which the adherents of this opinion denominate capital, consists merely in the use of those things obtained in return—so, the profit of the other part of stock, the portion reserved for consumption, is the use to which it is immediately subservient. According to Dr. Smith, there is some difference between revenue and enjoyment; and that part of a man's property yields him no profit, which is most useful and necessary to him, by which he can support and enjoy life without the necessity of any operation of barter.

Labour, on the other hand, is so far different in the mode of its subserviency to our enjoyments, that it can in no

way be ranked in the same class, either with capital or with land. Labour is applicable to both land and capital. It is the means of rendering them useful, or of increasing their utility. It is truly the origin and source of wealth; but is, in no sense of the word, wealth itself—unless, indeed, we conceive the pleasure of some kinds of exertion to be a use of labour analogous to the enjoyment of riches. Wealth may be said to be every thing from which man immediately derives the supply of his wants and desires. Its component parts are as various as those wants and desires, though it is, no doubt, susceptible of various general divisions, liable to no just exceptions in point of accuracy. Thus, it may be ranged in the two classes of matter and mind, or property and talents; and property may be divided into animate and inanimate, or the lifeless and the living, things over which man has dominion. By a combination of those component parts of wealth—by the operation of talents on property, and by a combination of the component parts of property—by the operation of living powers upon inert matter, man is enabled to increase the whole of his possessions, and to augment the sum of his enjoyments. In by far the greater number of instances, some exertion of labour is necessary to profit by his possessions; but this is not universally the case, unless we go so far as to term that exertion labour, which consists in the very act of enjoyment, or of use; for it would scarcely be correct, to consider the eating of wild fruits on the tree as the labour paid for the acquisition of them; it is rather the enjoyment of them—and has nothing in it analogous to the previous exertion required to procure similar fruits by culture, and which must be followed by the same exertion in using them.

III.

I have now before me a number of Dr. Smith's Letters, written when at Oxford, between the years 1740 and 1746, to his mother: they are almost all upon mere family and personal matters; most of them indeed upon his linen and other such necessaries, but all show his strong affection for his parent. Writing 2d July, 1744, he says:—

"I am quite inexcusable for not writing to you oftener. I think of you every day, but always defer writing till the

post is just going, and then sometimes business or company, but oftener laziness, hinders me. Tar water is a remedy very much in vogue here at present for almost all diseases. It has perfectly cured me of an inveterate scurvy and shaking in the head. I wish you'd try it. I fancy it might be of service to you." In another letter he says he had had the scurvy and shaking as long as he remembered anything, and that the tar water had not removed those complaints.

29th November, 1743.—"I am just recovered of a violent fit of laziness, which has confined me to my elbow-chair these three months."

It should seem as if his habitual absence had assumed a marked form at that time. The description resembles that of a hypochondriacal malady. He was then only twenty years old.

I have likewise had access to some letters which he wrote afterwards to Lord Hailes, and, through the kindness of the Royal Society of Edinburgh, to such of his letters as are in the papers of David Hume.

The following letter to Lord Hailes, dated 5th March, 1769, gives the germ of some of his speculations, but it is also curious as giving his very strong and very rash opinion against the decision of the great Douglas Cause.

"My Lord, Kirkaldy, March 5, 1769.

"I should now be extremely obliged to your Lordship if you would send me the papers you mentioned upon the prices of provisions in former times. In order that the conveyance may be perfectly secure, if your Lordship will give me leave, I shall send my own servant sometime this week to receive them at your Lordship's house at Edinburgh. I have not been able to get the papers in the cause of Lord Galloway and Lord Morton. If your Lordship is possessed of them it would likewise be a great obligation if you could send me them. I shall return both as soon as possible. If your Lordship will give me leave I shall transcribe the MSS. papers; this, however, entirely depends upon your Lordship.

"Since the last time I had the honour of writing to your Lordship, I have read over with more care than before the

Acts of James Ist, and compared them with your Lordship's remarks. From these last I have received both much pleasure and much instruction. Your Lordship's remarks will, I plainly see, be of much more use to me than, I am afraid, mine will be to you. I have read law entirely with a view to form some general notion of the great outlines of the plan according to which justice has been administered in different ages and nations; and I have entered very little into the detail of particulars of which I see your Lordship is very much master. Your Lordship's particular facts will be of great use to correct my general views; but the latter I fear will always be too vague and superficial to be of much use to your Lordship.

"I have nothing to add to what your Lordship has observed upon the Acts of James Ist. They are penned in general in a much ruder and more inaccurate manner than either the English statutes or French ordinances of the same period; and Scotland seems to have been, even during this vigorous reign, as our historians represent it, in greater disorder than either France or England had been from the time of the Danish and Norwegian incursions. The 5, 24, 56, and 85 statutes, seem all to attempt a remedy to one and the same abuse. Travelling, from the disorders of the country, must have been extremely dangerous, and consequently very rare. Few people, therefore, could propose to live by entertaining travellers; and consequently there would be few or no inns. Travellers would be obliged to have recourse to the hospitality of private families in the same manner as in all other barbarous countries; and being in this situation real objects of compassion, private families would think themselves obliged to receive them, even though this hospitality was extremely oppressive. Strangers, says Homer, are sacred persons, and under the protection of Jupiter; but no wise man would ever choose to send for a stranger unless he was either a bard or a soothsayer. The danger, too, of travelling either alone or with few attendants made all men of any consequence carry along with them a numerous suite of retainers, which rendered this hospitality still more oppressive. Hence the orders to build hostellaries in 24 and 85. And as many people had chosen to follow the old fashion and to live rather at the expense of other

people than at their own, hence the complaint of the keepers of the hostellaries, and the order thereupon in Act 56.

"I cannot conclude this letter, though already too long, without expressing to your Lordship my concern, and, still more, my indignation at what has lately passed both at London and at Edinburgh. I have often thought that the Supreme Court of the United Kingdom very much resembled a jury. The law Lords generally take upon them to sum up the evidence, and to explain the law to the other peers, who generally follow their opinion implicitly. Of the two law Lords who upon this occasion instructed them, the one has always run after the applause of the mob; the other, by far the most intelligent, has always shown the greatest dread of popular odium, which, however, he has not been able to avoid. His inclinations also have always been suspected to favour one of the parties. He has upon this occasion, I suspect, followed rather his fears and his inclinations than his judgment. I could say a great deal more upon this subject to your Lordship, but I am afraid I have already said too much. I would rather, for my own part, have the solid reputation of your most respectable President, though exposed to the insults of a brutal mob, than all the vain and flimsy applause that has ever yet been bestowed upon either or both the other two. I have the honour to be, with the highest esteem and regard,

"My Lord,
"Your Lordship's most obliged
and obedient servant,
(Signed) "ADAM SMITH."

Another letter, dated a week later, gives what is evidently the beginning of his speculations on the price of silver, and adds as to the Douglas Cause—

"If the rejoicings which I read of in the public papers in different places on account of the Douglas Cause had no more foundation than those which were said to have been in this place, there has been very little joy upon the occasion. There was here no sort of rejoicing of any kind, unless four schoolboys having set up three candles upon the trone, by way of an illumination, is to be considered as such."

In one of his letters to Mr. Hume, from Toulouse, he complains much of the dull life he led from not having brought introductions to society. "The life (he says) which I led at Glasgow was a pleasurable dissipated life in comparison of that which I lead here. I have begun to write a book in order to pass away the time: you may believe I have very little to do." This letter is dated 5th July, 1764, and the work was plainly the 'Wealth of Nations.' The mention of it is interesting, as being the first we have of his great undertaking. I need hardly add, that from his habitual aversion to write letters, very few remain of his compared with the correspondence of most distinguished men. Afterwards he lived in all the society of Toulouse. Here is another letter of a later date on Mr. Hume's quarrel with Rousseau:—

"MY DEAR FRIEND, PARIS, July 6th, 1766.

"I am thoroughly convinced that Rousseau is as great a rascal as you and as every man here believes him to be; yet let me beg of you not to think of publishing any thing to the world upon the very great impertinence which he has been guilty of to you. By refusing the pension which you had the goodness to solicit for him with his own consent, he may have thrown, by the baseness of his proceedings, some little ridicule upon you in the eyes of the Court and the Ministry. Stand this ridicule; expose his brutal letter, but without giving it out of your own hand, so that it may never be printed, and if you can, laugh at yourself, and I shall pawn my life that before three weeks are at an end, this little affair, which at present gives you so much uneasiness, shall be understood to do you as much honour as any thing that has ever happened to you. By endeavouring to unmask before the public this hypocritical pedant, you run the risk of disturbing the tranquillity of your whole life. By letting him alone he cannot give you a fortnight's uneasiness. To write against him is, you may depend upon it, the very thing he wishes you to do. He is in danger of falling into obscurity in England, and he hopes to make himself considerable by provoking an illustrious adversary. He will have a great party, the Church, the Whigs, the Jacobites, the whole wise English nation, who

will love to mortify a Scotchman, and to applaud a man that has refused a pension from the King. It is not unlikely, too, that they may pay him very well for having refused it, and that even he may have had in view this compensation. Your old friends here wish you not to write —the Baron d'Alembert, Madame Riccoboni, Mademoiselle Riancourt, M. Turgot, &c., &c. M. Turgot, a friend every way worthy of you, desired me to recommend this advice to you in a particular manner, as his most earnest entreaty and opinion. He and I are both afraid that you are surrounded with evil counsellors, and that the advice of your English literati, who are themselves accustomed to publish all their little gossiping stories in newspapers, may have too much influence upon you. Remember me to Mr. Walpole, and believe me to be, with the most sincere affection,

"Ever yours,

"ADAM SMITH."

"P.S. Make my apology to Miller for not having yet answered his last very kind letter. I am preparing the answer to it, which he will certainly receive by next post. Remember me to Mrs. Miller. Do you ever see Mr. Townshend?"

After his return to Kirkaldy, and when engaged in his great work he thus writes—

"MY DEAREST FRIEND, KIRKALDY, June 7th, 1767.

"The principal design of this letter is to recommend to your particular attention the Count de Sarsfield, the best and the most agreeable friend I had in France. Introduce him, if you find it proper, to all the friends of your absent friend, to Oswald and to Elliot in particular. I cannot express to you how anxious I am that his stay in London should be rendered agreeable to him. You know him, and must know what a plain, worthy, honourable man he is. I have enclosed a letter for him, which you may either send to him, or rather, if the weighty affairs of state will permit it, deliver it to him yourself. The letter to Dr. Morton you may send by the penny post.

"My business here is study, in which I have been very deeply engaged for about a month past. My amusements are long solitary walks by the sea side. You may judge how I spend my time. I feel myself, however, extremely happy, comfortable, and contented. I never was perhaps more so in all my life. You will give me great comfort by writing to me now and then, and by letting me know what is passing among my friends at London. Remember me to them all, particularly to Mr. Adams's family and to Mrs. Montague.

"What has become of Rousseau? Has he gone abroad, because he cannot contrive to get himself sufficiently persecuted in Great Britain?

"What is the meaning of the bargain that your Ministry have made with the India Company? They have not I see prolonged their Charter, which is a good circumstance. What are you going to do?"*

Thinking it probable that the Dalkeith repositories might contain some letters, the present Duke of Buccleugh was kind enough, at my request, to make search, but none were found.

I have much satisfaction in adding the following letter, because it gives Dr. Smith's first impressions, which in this case proved most just ones, of a person whose virtues and amiable qualities were the theme of universal respect and esteem during her whole life, the late Duchess of Buccleugh, grandmother of the present Duke.

"My dear Friend, Dalkeith House, September 18, 1767.

"Be so good as to convey the enclosed letter to the Count de Sarsfield; I have been much in the wrong for having delayed so long to write both to him and you.

"There is a very amiable, modest, brave, worthy young gentleman, who lives in the same house with you; his name is David Skeene. He and I are sisters' sons, but my regard for him is much more founded upon his personal qualities than upon the relation in which he stands to me. He acted lately in a very gallant manner in America, of which he never acquainted me himself, and of which I came to the

* Remainder of the letter obliterated.

knowledge only within these few days. If you can be of any service to him, you could not possibly do a more obliging thing to me. The Duke and Duchess of Buccleugh have been here now for almost a fortnight; they begin to open their house on Monday next, and I flatter myself will both be very agreeable to the people of this country. I am not sure that I have ever seen a more agreeable woman than the Duchess. I am sorry that you are not here, because I am sure you would be perfectly in love with her. I shall probably be here some weeks; I would wish, however, that both you and the Count de Sarsfield would direct for me as usual at Kirkaldy. I should be glad to know the true history of Rousseau before and since he left England. You may perfectly depend upon my never quoting you to any living soul upon that subject.

"I ever am, dear Sir,

"Most faithfully yours,

"ADAM SMITH."

The following letter relates to his unhappy determination of having all his papers destroyed.

"MY DEAR FRIEND, EDINBURGH, April 16th, 1773.

"As I have left the care of all my literary papers to you, I must tell you that, except those which I carry along with me, there are none worth the publishing but a fragment of a great work, which contains a history of the Astronomical Systems that were successively in fashion down to the time of Des Cartes. Whether that might not be published as a fragment of an intended juvenile work I leave entirely to your judgment, though I begin to suspect myself, that there is more refinement than solidity in some parts of it. This little work you will find in a thin folio paper book, in my writing-desk in my book-room; all the other loose papers, which you will find either in that desk or within the glass folding doors of a bureau, which stands in my bed-room, together with about eighteen thin paper folio books, which you will likewise find within the same glass folding doors, I desire may be destroyed without any examination. Unless I die very suddenly, I shall take care

that the papers I carry with me shall be carefully sent to you.

"I ever am, my dear friend,
"Most faithfully yours,
"ADAM SMITH."

"To DAVID HUME, Esq.,
of St. Andrew's Square, Edinburgh."

"MY DEAREST FRIEND, KIRKALDY, August 22nd, 1776.

"I have this moment received your letter of the 15th instant. You had, in order to save me the sum of one penny sterling, sent it by the carrier instead of the post; and (if you have not mistaken the date) it has lain at his quarters these eight days, and was, I presume, very likely to lie there for ever.

"I shall be very happy to receive a copy of your Dialogues; and, if I should happen to die before they are published, I shall take care that my copy shall be as carefully preserved as if I was to live a hundred years. With regard to leaving me the property in case they are not published within five years after your decease, you may do as you think proper. I think, however, you should not menace Strahan with the loss of any thing in case he does not publish your Work within a certain time.* There is no probability of his delaying it, and if any thing could make him delay it, it would be a clause of this kind; which would give him an honourable pretence for doing so. It would then be said that I had published, for the sake of an Establishment, not from respect to the memory of my friend, what even a Printer for the sake of the same emolument had not published. That Strahan is sufficiently zealous you will see by the enclosed letter, which I will beg the favour of you to return to me, but by the post and not by the carrier. If you will give me leave I will add a few lines to your account of your own Life; giving some account in my own name, of your behaviour in this illness, if, contrary to my own hopes, it should prove your last. Some conversations we

* This refers to the passage of Mr. Hume's will, imposing a penalty in case of not printing one of his posthumous works. See 'Life of Hume,' vol. i.

had lately together, particularly that concerning your want of an excuse to make to Charon, the excuse you at last thought of, and the very bad reception which Charon was likely to give it, would, I imagine, make no disagreeable part of the history. You have in a declining state of health, under an exhausting disease, for more than two years together, now looked at the approach, or what you at least believed to be the approach of Death with a steady cheerfulness such as very few men have been able to maintain for a few hours, though otherwise in the most perfect health. I shall likewise, if you will give me leave, correct the sheets of the new edition of your Works, and shall take care that it shall be published exactly according to your late corrections. As I shall be at London this winter it will cost me very little trouble. All this I have written upon the supposition that the event of your disease should prove different from what I still hope it may do. For your spirits are so good, the spirit of life is still so very strong in you, and the progress of your disorder is so slow and gradual, that I still hope it may take a turn. Even the cool and steady Dr. Black, by a letter I received from him last week, seems not to be averse to the same hopes.

"I hope I need not repeat to you, that I am ready to wait on you whenever you wish to see me. Whenever you do so, I hope you will not scruple to call on me. I beg to be remembered in the kindest and most respectful manner to your Brother, your Sister, your Nephew, and all other Friends.

"I ever am,

"My dearest friend,

"Most affectionately yours,

"ADAM SMITH."

TO JOHN HOME, OF NINEWELLS.

"DEAR SIR, "DALKEITH HOUSE, August 31st, 1776.

"As the Duke proposes to stay here till Thursday next, I may not have an opportunity of seeing you before you return to Ninewells; I, therefore, take this opportunity of discharging you, and all others concerned, of the legacy which you was so good as to think might, upon a certain event, become due to me by your brother's will, but which,

I think, would upon no event become so, viz., the legacy of two hundred pounds sterling. I hereby therefore discharge it for ever; and least this discharge should be lost, I shall be careful to mention it in a note at the bottom of my will. I shall be glad to hear that you have received this letter, and hope you will believe me to be, both on your brother's account and your own, with great truth, most affectionately,

"Yours,

"ADAM SMITH.

"P.S.—I do not hereby mean to discharge the other legacy, viz., that of a copy of his works."

"DEAR SIR, "EDINBURGH, September 2nd, 1776.

"I was favoured with your's of Saturday, and I assure you that, on perusing the destinations, I was more of opinion than when I saw you, that the pecuniary part of it was not altered by the codicil, and that it was intended for you at all events; that my brother knowing your liberal way of thinking, laid on you something as an equivalent, not imagining you would refuse a small gratuity from the funds it was to come from, as a testimony of his friendship; and though I must highly esteem the motives and manner, I cannot agree to accept of your renunciation, but leave you full master to dispose of it which way is most agreeable to you.

"The copys of the Dialogues are finished, and of the Life, and will be sent to Mr. Strahan to-morrow; and I will mention to him your intention of adding to the last something to finish so valuable a life, and will leave you at Liberty to look into the correction of the first, as it either answers your leisure or ideas with regard to the composition, or what effects you think it may have with regard to yourself. The two copys intended for you will be left with my sister, when you please to require them; and the copy of the new edition of his works you shall be sure to receive, though you have no better title to that part than the other, though much you have to the friendship and esteem of, Dear Sir, him who is most sincerely,

"Yours,

"JOHN HOME."

LAVOISIER.

In the Lives of Black, Priestley, Watt, and Cavendish, it has been necessary to mention the claims of Lavoisier, first as a competitor with the great philosophers of the age for the honour of their discoveries, yet as an intruder among them by his attempts to show that he had himself, though unknown to them and ignorant of their inquiries, made the same steps nearly at the same time. The history of that great man, which we are now to consider, will enable us to perceive clearly the evidence upon which the charge rests, both the proof of his having preferred those claims, and the proof that they were groundless. But it will also enable us to perceive how vast his real merits were, and how much remained his own of the discoveries which have built up the science of modern chemistry, even after all those plumes have been stript away that belonged to others.

It is a very great error to suppose that the truths of philosophy are alone important to be learnt by its students; that provided these truths are taught, it signifies little when or by whom or by what steps they were discovered. The history of science, of the stages by which its advances have been made, of the relative merits by which each of our teachers was successively made famous, is of an importance far beyond its being subservient to the gratification even of an enlightened and learned curiosity. It is eminently calculated to further the progress which it records; it conveys peculiarly clear and discriminating ideas upon the doctrines taught, and the proofs they rest on; it suggests new inquiries, and encourages the prosecuting of new re-

searches. It is, moreover, both a debt of gratitude to our benefactors which we should be anxious to pay by testifying our gratitude, and commemorating their fame; and the discharge of this duty has a direct tendency to excite emulation, prompting to further labours that may enlarge the bounds of science. Besides, the history of scientific achievements is the history of the human mind in its noblest exertions, of the human race in its most exalted pursuits. But it is equally clear that the whole value of this, as of every other branch of history, depends upon the diligence with which the facts are examined, the care and even the skill with which their evidence is sifted, the impartiality with which judgment is pronounced, and the accuracy with which the record is finally made up. The mere panegyric of eminent men, how elegantly soever it may be composed, must remain wholly worthless, at the best, and is capable of being mischievous, if it aims at praise without due discrimination, still more if it awards to one man the eulogy which of right belongs to another. Nothing can be more indispensable to the execution of the important task undertaken by the historian of science, than that he should most carefully examine the share which each of its cultivators had in the successive changes it has undergone. The greatest of these have ever felt how valuable such titles are, and have shown the most singular anxiety to compare and to adjust their relative claims. Of these illustrious men I have known two, Black and Watt, and I can safely say that when the question was raised of priority in discovery among either their predecessors or their cotemporaries, they were wont to be particular and minute, even to what seemed superfluous carefulness, in assigning to each his just share, very far more anxious in making this distribution than they ever showed themselves to secure the admission of their titles in their own case. By a singular injustice of fortune these two philosophers have themselves

been treated with a more scanty measure of the like justice than perhaps any of their cotemporary discoverers.* It is proposed to examine with the same minuteness the particulars in M. Lavoisier's history, upon which some controversy has at different times arisen.

Antoine Laurent Lavoisier was born at Paris, 13th of August, 1743, the son of an opulent family, his father having been a fermier-général. No expense was spared upon his education; and in the college of Mazarin, where he studied, he gained many prizes for proficiency in classical acquirements. It was, however, to the sciences that he soon devoted himself, and first to the severer ones, having made considerable proficiency in the mathematics and astronomy under La Caille, in whose observatory he studied upon leaving the college. He studied botany under Jussieu, and chemistry under Rouelle. As from his earliest years he appears to have been wholly consecrated to scientific pursuits, so no one ever entered upon his course with a more fervid courage. The earliest of his inquiries of which we have any knowledge was an analysis of gypsum, presented to the Academy of Sciences in 1765, and published in the collection of 'Mémoires de divers Savans,' 1768. In 1764 a prize had been proposed by M. de Sartine, the celebrated chief of the police of Paris, for the best method of lighting a great town, so as to combine illumination with economy, and with facility of service. After the lapse of twelve months no dissertation had been presented which satisfied the conditions of the programme, and the prize was doubled, being raised to 2000 livres; and next year, 1766, the conditions remaining still unsatisfied by the candidates, the prize was divided

* When any reference is made to the Eloges of the French Academy, justice requires me to add that those of M. Arago form a most striking exception. They are strictly historical, as well as philosophical. That of Watt is a model.

among the three best, while a Memoir of great merit, by M. Lavoisier, was honourably mentioned and ordered to be printed. The King, too, on M. de Sartine's recommendation, directed a gold medal to be bestowed upon the author, who was presented with it at the public sitting of the Academy in April, 1766. In 1769 he obtained the place of a fermier-général, by a kind of hereditary title; and in 1771 he married Marie-Anne Paulze, whose father likewise belonged to the same financial class. In 1768 he had been admitted a member of the Academy, at the early age of twenty-five. His paper on the lapis specularis, related to the composition of the great strata forming the basin of Paris.

He appears for some years to have occupied himself principally with geological inquiries; he made mineralogical journeys in various parts of France in company with M. Guettard; and he had collected materials for an extensive work on the revolutions of the globe, when the recent progress of another science gave a new direction to his pursuits. His paper on gypsum contains a number of experiments, which show it to be a neutral salt, soluble in a great proportion of water, and composed of sulphuric acid united to a calcareous base. This and almost every other position in his paper was well known before. M. Montigny had, in the 'Memoirs of the Academy,' 1762, shown its solubility, and M. Margraaff, in the 'Berlin Memoirs,' as far back as 1750, had proved both this and its composition. M. Lavoisier refers to these long-published works in a note appended to his paper, but states that he had not seen Margraaff's till after his own was read before the Academy. He also states that M. Baumè had published researches similar to his in a journal, but that he was not aware of this till he had made considerable progress with his paper. It is unfortunate that this eminent person should have begun his works with this kind of doubt hanging over his origi-

nality. Yet we may observe that his paper contains an ingenious theory, explaining the phenomenon of the formation of gypsum on the principles of ordinary crystallization; and that he has also ascertained the proportion of water required for its solution more accurately than had before been done; that he gave a systematic view of the whole subject. *Qualis ab incepto processerat.* It is remarkable that all the distinguishing characters of his inquiries in after-times should be found to mark this his first production. We observe the same disputed originality in his experiments, the same anticipation of his discoveries by former inquirers, the same superiority of his processes in point of accurate admeasurement, the same inferiority of his experiments to his reasonings, the same happy generalization of facts observed by others, the same turn for throwing doctrines and discoveries not his own into one combined system.

The discoveries of Black had, long before M. Lavoisier entered upon his scientific pursuits, directed the attention of philosophers to the important subject of gaseous bodies, to their production by the absorption of heat, and to the combinations into which they enter with other substances, so as to alter the nature of these. The great doctrines of causticity and of latent heat, with the existence of fixed air, and its evolution in respiration, fermentation, and combustion, had been established, and had formed a new era in chemical science. Fixed air was discovered in 1754; latent heat before 1763. Mr. Cavendish had prosecuted these inquiries with success; he had examined some of the properties both of fixed air and of hydrogen; had determined their specific gravities, and had shown that they are always the same from whatever substances they may be obtained. His experiments were published in 1766. Soon after this time Dr. Priestley began his brilliant course of discovery. A new scene had been opened to philosophers; they were like

infants gazing on the material world, every object of which is new to them, and whose whole existence is one continued gratification of curiosity. Aware from former discoveries that various kinds of air, each having its peculiar properties, exist in nature, he was of course ever expecting to meet with them; and, accordingly, he soon found that the air of the atmosphere yields one of these, which on a false theory he termed phlogisticated, but which others have termed azote, being incapable of supporting either animal life or flame. These experiments of his were published in 1772.

Before proceeding further with the history of chemical discovery, it is necessary I should mention a serious inconvenience thrown in the way of the accurate inquirer by the very extraordinary manner in which the 'Memoirs of the French Academy' have always been published. The 'Philosophical Transactions' appear most regularly in two, sometimes, though very rarely, in three parts every year, and all the papers published each year have been read before the Society during the course of that year; nay, all the papers which form each part have been read during the half-year immediately preceding the publication of that part. It is far otherwise with the French Academy's 'Memoirs;' these never are published in less than three, sometimes even four years after the year to which they nominally relate. Thus the volume for 1772 consists of two parts, one of which was published in 1775, and the other in 1776. But this would occasion a small inconvenience to the inquirer into dates and facts, if it only indicated that the work was constantly in arrear, and that the papers purporting to be those of any given year, as 1772, were not published till three or four years later. That, however, is by no means the case. It continually happens that the papers classed as those of one year were in reality read a year or two later. In earlier periods the dates are often not given at which papers were read, but from internal evidence we find when they were

read; for in the volume for 1772, p. 12, we have M. Lavoisier quoting a book published in January, 1773, and describing an experiment made in August of that year, (p. 598). So in the volume for 1770, we have an account of an eclipse in April, 1771, and of experiments made in autumn, 1771, (p. 621). In later volumes the dates are more accurately given, though sometimes they tend to bewilder us. Thus the volume for 1776 was not published till 1778, and it contains a paper of M. Lavoisier, printed in Sept. 1778, and read 23rd Nov., 1779. So the volume for 1776 contains another paper of his, stated to have been printed in Dec., 1777. In like manner the volume for 1774 was published in 1778, and it contains a paper read 1774, but *relû* 1777. And the volume for 1775 has a paper read Easter, 1775, *relû* Aug., 1778. It is needless to remark how very difficult this kind of confusion and inaccuracy, wholly unaccountable, renders it to ascertain the precise date at which any experiment was made, or theory formed. We are in most cases left to mere conjecture, being uncertain of anything but the time of publication, and not always sure of that.

In the year 1768 M. Lavoisier began to occupy himself almost exclusively with chemical inquiries. Well educated in the kindred branches of natural philosophy, and fully conversant with all that was then known of chemistry, ardent in the pursuit of scientific truth, filled with a noble ambition to distinguish himself among its students, careless of the various pursuits which men in his circumstances find all-engrossing, he was also in possession of ample wealth, and could both command the aid of some and obtain the fellowship of others in his researches, while the most costly apparatus, and the most expensive experiments, were at all times within his reach. He soon filled his house with the finest instruments, and opened it freely to all men of letters and of science. In their company, and with the inestimable advantage of their constant society, in which every

point was discussed and all difficulties encountered by their lights as well as his own, he devoted the rest of his praiseworthy life to his favourite science, repeating the experiments of others, varying them with the suggestions of his own mind, and, in some instances, devising new ones which he successfully conducted. We are now to consider the fruits of these glorious labours.

In 1768 and 1769 he made a number of very laborious and very accurate experiments, with the view of ascertaining the correctness of an opinion long entertained, and among others by Bonde and Margraaff, that water may, by repeated distillations, be converted into earth; and also of determining whether or not there was any foundation for the opinion that water can, by repeated distillations, become so elastic and aëriform as to escape through the pores of vessels: an opinion entertained by Stahl, the celebrated author of the phlogistic theory. M. Lavoisier satisfactorily disproved both these positions, and showed that the earth which had misled others was a portion of the vessels used in performing the distillation. The account of these experiments was given to the Academy in 1770, and published in 1773. It may give us some idea of the pains with which they were performed, to state that one of the processes lasted a hundred and one days.

In the year after these inquiries were carried on, his attention appears to have been turned aside from chemical studies, by the reports which he made to the Academy upon the means of supplying Paris with water, at an economical rate. A question having arisen between the Government and M. Parcieux, a learned mechanical projector, on the comparative expense of bringing the water of the rivulet Yvette by canal and wheel engines, or by steam engine, M. Lavoisier examined the subject, and showed that the latter mode was the most expensive. His Memoir appeared in the

volume for 1771. In that year, however, he resumed his chemical pursuits, and applied himself to the attentive consideration of the calcination of metals. The recent discoveries on the nature of gases by Black, Cavendish, and Priestley, appear to have chiefly contributed to his doubts upon the foundation of Stahl's theory, which considers the union of phlogiston, or the matter of heat and light, with the basis of the metals, as the cause of their lustre and ductility, and the evolution of that substance as the cause of their becoming earthy, or calces. M. Lavoisier examined the process by which minium, or red lead, is reduced, that is, resumes its metallic state, and he found that there was always evolved a great quantity of air, which he examined and found to be fixed air, being, he expressly says, the same that escapes in the effervescence of alkalis and calcareous earth, and in the fermentation of liquors. He then examined the converse operation of calcination, and found it accompanied with an absorption of air, and that the weight of the metal had increased by the whole weight of the air absorbed. The inference which he drew was, that calcination is caused by the union of air with the metal, and not by the loss of any body, as phlogiston, combined with it. These experiments and this theory he published at the end of the year 1773, in a small volume entitled 'Opuscules Physiques,' which describes very fully the previous discoveries on gases and on heat, and contains many ingenious discussions on the processes of calcination and combustion. He had in the course of that year read several Memoirs, on the subject of his own experiments, to the Academy, and had shown these experiments to several of its members. Nothing, therefore, can be more incontestable than his claim to the important step now made the cause of so many others, that the calcination of metals is their uniting with a gas become fixed and solid in their substance; and a mortal blow was thus given to the theory of

Stahl.* But it must be added that he was wholly ignorant of the nature of the air absorbed. He seems to have been deceived by the quantity of fixed air which minium contains, and to have hastily supposed this air to be the cause of calcination, without examining the air in which he performed the more useful and converse experiment.

It is singular how very near M. Lavoisier came in these inquiries to two discoveries of first-rate importance. He could not have examined with any care the residue of the air in which his calcinations were performed, without discovering the composition of the atmosphere; nor could he have fully examined the air given out in the reduction of calces to their reguline, or metallic state, without discovering oxygen. It was reserved for Dr. Priestley, two years later, to make both these capital discoveries.

A similar remark arises upon the next inquiry of any importance in which M. Lavoisier was engaged. For we may pass over his experiments on the use of alcohol in the analysis of mineral waters, as he admits that the subject was familiar to chemists, having been treated at length by Macquer. It may, however, be observed in passing, that he claims as a discovery the proposition that alcohol attacks salts differently when mixed with different proportions of water; and also, that nothing can be more crude than his notions of the connections between the salts and the mineral kingdom—for a large portion of his Memoir is devoted to prove that there can only be three mineral alkalis, soda, calcareous earth, and what he calls the base of Epsom salts, which is magnesia, and two mineral acids, the vitriolic and muriatic. These propositions were as wide of the truth as possible, and, apparently, were

* It is truly painful to find the determination of French writers never to take the trouble of giving the names of foreigners with any accuracy. Lavoisier always calls Stahl either Stalh or Sthal, and never once gives his right name.

chiefly recommended to him by their showing that the experiments with alcohol, which he had made with those substances, exhausted the subject of mineral waters.

But the next important inquiry of this eminent chemist related to the action of heat on the diamond, or, as he very inaccurately termed it, the destruction of the diamond by fire. These experiments were performed with great care, and without any regard to expense; to which purpose a public-spirited jeweller also contributed largely. They were performed partly by fire, partly by the great lens of Tschirnausen belonging to the Academy. The Memoir is in the volume for 1772, Part II., published in 1776; but the experiments were not all performed till late in 1773, and the Memoir was probably read in 1774. It was found that some carbonaceous effervescence (as he describes it) could be observed when the heat applied was not very strong, though a stronger heat dissipated the diamond altogether if it was exposed to the air. Hence M. Lavoisier inferred, that beside being a combustible substance, as Newton had sagaciously imagined from its optical qualities, and as Macquer had proved by direct experiment, it is capable of conversion into charcoal. But a more important fact was also ascertained. M. Lavoisier examined the air in which the evaporation, as he terms it, of the diamond was performed, and he found that it precipitated lime from lime water. Examining the lime thus thrown down he found it to be chalk, and thence concluded most justly that the air produced during the combustion of the diamond was fixed air. This, however, is not his enunciation of the proposition; he only says, that the air in which the diamond had been evaporated had acquired in part the properties of fixed air, or the air which, he correctly says, comes from the effervescence of alkalis and from fermentation, and which, he very erroneously says, (following the mistake into which he had fallen in his experiments on calcination) is the air

given out by metallic calces on their reduction to the reguline state. He rests in doubt between the two inferences from his experiments—the one, that the diamond evaporates into fixed air; the other, that its vapour changes atmospheric into fixed air.

Observing the analogy between the diamond and combustible bodies, he exposed it to heat when surrounded with fixed air, and atmospheric air was excluded. The evaporation went on, but much more difficultly and slowly. The probability is that the air was not entirely fixed air, else the diamond could not have evaporated at all.

The production of fixed air by burning charcoal, alcohol, ether, in close vessels had been long known; but M. Lavoisier carefully subjected charcoal to the same process which he had made the diamond undergo, and the result was nearly the same.

The conclusion at which he arrived from these experiments, is marked by a caution truly philosophic, and as well deserving our admiration, as the sagacity which distinguished the conduct of the inquiry. "We should never have expected," he says, "to find any relation between charcoal and diamond, and it would be unreasonable to push this analogy too far; it only exists because both substances seem to be properly ranged in the class of combustible bodies, and because they are of all these bodies the most fixed when kept from the contact of air." He adds, "It is far from being impossible that the blackish matter should come from surrounding bodies, and not from the diamond itself."

It is needless to remark how very near he was, in this inquiry, to making the discovery that diamond and the pure carbonaceous matter are identical, and that both form alike fixed air by their union with another and a gaseous substance. Dr. Black had shown, nearly twenty years before, that fixed air was the product of the combustion of charcoal. Had M. Lavoisier performed his experiments on that combustion with a little

more care, he would have made the discovery in 1773, which he did a few years later; and as he then was occupied in considering the nature of the diamond, its identity with carbon would not have escaped him as it afterwards did when he first ascertained the composition of fixed air.

In 1773, M. Lavoisier made some very accurate experiments upon the calcination of tin in close vessels; and he proved clearly that the whole air and metal after calcination weighed exactly the same as before, and that the metal had gained in weight exactly what the air had lost. But he adds an inference which is very remarkable on more accounts than one. It is that the atmosphere is composed of two gases, one capable of supporting life and flame, and of combining with metals in their calcination, the other incapable of supporting either life or flame, or of combining with metals. Now here begins the blame imputable to this great philosopher. His paper is said in his Memoir (p. 351,) to have been read at Martinmas, 1774; and to have been "*relû*," 10 May, 1777; he says, p. 366, that he had received a letter from P. Beccaria, dated 12 Nov. 1774, but that his own Memoir was then drawn up, and that an "Extract" of it had been read at the public sitting in November. He does not state whether or not the important doctrine above-mentioned, on the constituent parts of the atmosphere, was contained in that extract; nor how long before 10 May, 1777, it was added to the paper. Moreover, he says nothing whatever of the communication made to him by Dr. Priestley, in October, 1774, of his grand discovery of oxygen. Nor does he mention that the same philosopher had, in 1772, discovered the existence of azote in the atmosphere, and received, from our Royal Society, the Copley medal the following year, on account of his paper printed in the 'Philosophical Transactions for 1772.' It is wholly impossible to believe that the experiments on tin could have given M. Lavoisier any

light on the constitution of the atmosphere, which he had not derived from his similar experiments in 1770, and 1771, upon the reduction of minium, and the calcination of other metals. But the discoveries of Dr. Priestley must have been known to him in 1774; and what he gives as his conjectures derived from his own experiments, were the discoveries of Dr. Priestley in 1772 and 1774. The knowledge of these discoveries formed the only difference between the state of M. Lavoisier's information, when he experimented upon tin in 1773 and 1774, and when he experimented on lead three years before. It is perfectly clear that until the discoveries of Dr. Priestley, the chief of which, we have positive evidence, was communicated to him by the Doctor himself, he never had the least idea of the air absorbed in calcination possessing any qualities like those of oxygen gas, or had supposed that the air evolved in the reduction of calcined metals, was of that nature; indeed, he distinctly stated it to be fixed air, misled by the quantity of fixed air found in minium as an impurity. He had made many experiments on calces of metals, and he had never found any air to be contained in them resembling oxygen. Until he heard of Dr. Priestley's great experiment he never had thought of obtaining oxygen gas from those bodies, nor ever knew of the existence of that gas.

This is the plain inference from the history of his inquiries, as far as we have now followed it. But as he has himself, beside wrapping up the date of his theory in the general terms already observed when he presented his paper on tin, also laid positive claim to the discovery of oxygen in a subsequent Memoir, it becomes necessary to examine the grounds of this pretension more closely, and we shall find that this examination entirely confirms the position already stated, namely, his ignorance of oxygen, until the true discoverer made him acquainted with it.

We shall first give the words in which he couches

his claim. I quote from his 'Elémens de Chimie.' "Cet air" (oxygen gas,) "nous avons découvert presqu'en même tems, Dr. Priestley, M. Scheele et moi."

Now I begin this statement by observing, that as to the precise time of Dr. Priestley's discovery there is no doubt; no "presqu'en même tems;" it was the first day of August, 1774. Scheele, without knowing of his discovery, made the same the year after, 1775. So far then the statement of Lavoisier is incorrect; Priestley and Scheele did *not* discover oxygen, "presqu'en même tems." But we must proceed, and shall first of all examine in what way M. Lavoisier preferred his claim. For that would have rested upon a foundation somewhat more plausible had he brought it forward early, and always adhered to the same statement. But the reverse is the fact.

We must first remark that not a hint is dropped of this claim, in the paper upon calcination originally presented in 1774, and afterwards with additions in 1777. In 1775, at Easter, he read a paper on the nature of calcination, which was "relû 8 August, 1778;" with what additions is not stated. But the experiments which it contains are of two classes; the one set he says were made above a year before, or in spring 1774, and these throw no new light at all on the subject; the others were made, he says, first in November, 1774, and more fully before other persons, in the following spring. These experiments show that the oxygen of the atmosphere is absorbed in calcination; and this conclusion is stated; but no claim whatever is made to the discovery of oxygen gas, although if discovered by him at all, it must have been in those experiments. He only calls it "the most respirable portion of the atmosphere." A most important admission is, however, made in a subsequent paper, 1782, that the experiments in which he made this step, were not those performed in 1774, but those performed in February, 1775, (Vol. for 1782, p. 458). In 1776 he

printed a Memoir on Nitrous Acid, in which ample justice is done to Dr. Priestley's discoveries, and the experiments recounted as made by M. Lavoisier, are admitted to have all been Dr. Priestley's suggestions; he himself only claiming to have drawn more correct inferences from them. Among these inferences, there is only the one that nitrous acid consists of oxygen and nitrous gas; but no suspicion of its real composition, afterwards discovered by Mr. Cavendish to be the union of azote and oxygen, is even hinted at. It is also material to note, that in this paper not a word is said of the claim to having discovered oxygen. In 1777 a paper was printed by him on the combustion of phosphorus with "air éminemment respirable," to form phosphoric acid; that air is said to be "by Dr. Priestley termed dephlogisticated air," and still nothing is said of the claim to its joint discovery; but in p. 187 he speaks of the "expériences de Dr. Priestley et les miennes," on precipitate *per se.* These experiments, we are told by him, in the volume for 1775, (p. 520,) were made in November, 1774. In 1778, he printed, it is said, his Memoir on Acids. The date of presentation is given as September, 1778, but the reading is said to have been 23 November, 1779. In this paper, (p. 536,) he speaks of "the pure air to which Priestley gave the name of dephlogisticated, but which he himself calls oxygen, as being the acidifying principle." No mention is made of the base of nitrous acid, or of his claim to the discovery of oxygen. In 1780, in another paper, he speaks of "vital air, which Priestley improperly called dephlogisticated," (p. 336.) In the volume for 1781 is a paper on Scheele's work; and though Scheele's discovery of oxygen is mentioned, no claim to a partnership is advanced. In the same volume is the admirable paper on the constitution of fixed air, to which he gives the name of carbonic acid, but still no mention of having discovered oxygen. Thus we find that, in at least eight several papers

which discuss the effects produced by the absorption and the evolution of oxygen gas, printed between the years 1772 and 1780, not the least hint is given of his own claim, though in five of those papers he mentions Priestley as having given it a name; and one would therefore believe acknowledges him as the discoverer, without claiming any partnership for himself. This must be confessed to be a very strong circumstance, according to all the rules of evidence and principles of decision which men apply to the discussion and determination of claims in ordinary cases.

It was not till late in the year 1782, that this claim for the first time appeared. In a paper read November of that year, upon the means of increasing heat by the use of oxygen, he says, (p. 458,) "Cet air que M. Priestley a découvert à peu-près en même tems que moi, et je crois même avant moi;" and reminds the Academy that he had announced this inquiry at Easter, 1775, as having been conducted with M. Trudaine in Montigny's laboratory some months before. Now, in the Memoir already cited, he distinctly informs us that these experiments were not made till February, 1775; therefore, it is to this period that he refers his supposed discovery, and not to any part, however late, of 1774. It must also be borne in mind, that, for the reason formerly stated respecting the irregular publication of the Memoirs, and the inserting in one year the papers read long after, in many cases, without noting the date of their presentation, it becomes impossible to be certain of the time at which many of them were actually read. But I have always assumed that M. Lavoisier's were read at the times stated by him; and where no date is given I have supposed this paper to have been read in the year to which the volume refers—a supposition manifestly favourable, and often gratuitously favourable to his case.

We have thus seen the suspicious manner in which, after suffering to pass over at least eight occasions on

which he might naturally have brought forward the claim, he *at length* makes it at an interval of ten years; but he makes it with an important admission, that Priestley's discovery had been before his own. Yet strange to tell, when he repeats the assertion of "*presqu'en même tems*," in his 'Elémens de Chimie,' he entirely omits this statement of "et même je crois avant moi." Let us now observe what Dr. Priestley himself states, first remarking that he comes before us without the least unfavourable impression attached to his testimony, while M. Lavoisier's is subject to the weight of the observation already made, and arising entirely from his own conduct. Dr. Priestley, moreover, was a person of the most scrupulous veracity, and wholly incapable of giving any false colouring to the facts which he related respecting his discoveries. Indeed, no man ever showed less vanity respecting his extraordinary services to science. He even frankly and honestly, in the prefaces to his Essays, disclaims much merit that all men would allow him; and fairly tells how many of the great things which he had done were the suggestions of hazard, and not found out by any preconceived plan for making the discovery. No one, therefore, can possibly be cited whose authority is more unimpeachable in weighing the facts of such a case.—The following are his own words in a work published by him, in 1800, upon phlogiston. "The case was this. Having made the discovery (of oxygen) some time before I was in Paris, in the year 1774, I mentioned it at the table of M. Lavoisier, when most of the philosophical people of the city were present, saying, that it was a kind of air in which a candle burnt much better than in common air, but I had not then given it any name. At this all the company, and Mr. and Mrs. Lavoisier as much as any, expressed great surprise. I told them I had gotten it from *precipitate per se*, and also from *red lead*. Speaking French very imperfectly, and being little acquainted

with the terms of chemistry, I said *plombe rouge*, which was not understood till Mr. Macquer said I must mean *minium*. M. Scheele's discovery was certainly independent of mine, though, I believe, not made quite so early."

It is very important here to remark that M. Lavoisier's surprise was expressed at finding that *minium* had yielded this new air by reduction. He himself had made the experiment with minium, as we have seen, and only could detect fixed air as the produce; whence his erroneous inference that a metallic calx is composed of the metal united with fixed air. It was not till six months after this discovery of Dr. Priestley, and full four months after his expression of surprise, that he made the experiments which he many years afterwards thought it not unbecoming to affirm, had led him to the discovery about the same time with Priestley. I will venture to assert that no one, however little conversant with the rules of probability, or accustomed to weigh testimony, can hesitate a moment in drawing the conclusion, that M. Lavoisier never at any time made this discovery; that he intruded himself into the history of it, knowing that Priestley was its sole author; and that, in all likelihood, he covered over to himself this unworthy proceeding, so lamentable in the conduct of a truly great man, by the notion that he differed with Priestley in his theory of the gas—the one conceiving it to be a peculiar air deprived of phlogiston, and capable of taking it from inflammable bodies; the other holding it to be air which unites to inflammable bodies, and precipitates its heat and light in forming the union. But all must admit that the air was a newly discovered substance, a gas wholly different from all other gases formerly known; and that therefore, whatever might be the theory, the question of fact regarded the bringing this new substance to light. No self-deception, therefore, can vindicate M. Lavoisier for either the statement in his Memoir, suppressing all mention of

Dr. Priestley's communication, or the still more reprehensible statement in his 'Elements,' suppressing the hesitating confession of Priestley's priority.—With respect to Scheele the case is wholly different. What Priestley had discovered in 1774, he discovered the year following, without being aware that he had been anticipated. His process, too, was wholly different from Priestley's, whereas Lavoisier's was the very same. Of these great men, then, Priestley made the discovery in 1774, Scheele in 1775, Lavoisier neither in 1774 nor in 1775, nor ever except by receiving the information from "the true and first discoverer thereof, which, at the time, others did not use."*

There can be no doubt whatever that it was the discovery of oxygen gas which suggested to M. Lavoisier his theory of combustion. He had previously made the important step of explaining the calcination of metals, so far, at least, as showing that it was the union of the metals with the air absorbed, though he was wholly mistaken as to the air which they gave out on reduction, and had a most imperfect notion of the change which their calcination produced on the air in which the process took place; but now he was enabled, by Dr. Priestley's discovery, to show that the air absorbed is oxygen gas; while Dr. Black's great doctrine of heat, which he also called to his assistance, enabled him to perceive that the gas, on becoming fixed, parted with its latent heat, and assumed a solid form. A felicitous idea of Macquer's, which M. Lavoisier cites. ('Mem.,' 1777, p. 572,) that calcination is only a slow combustion, may have given rise to his theory of this operation; but he had also, in his experiments on phosphorus and sulphur, shown the absorption of oxygen by those bodies in burning; and as the doctrine of Dr. Black showed how much heat was evolved on a gaseous body becoming fixed and solid, we may

* Words of our Patent Act, 21 James I.

suppose that these experiments, which he laid before the Academy in the spring of 1777,* led him to his general theory. This theory is well known. It consists in supposing that all combustion, like all calcination, is produced by the union of oxygen with the body burnt or calcined; and that the gas which, in calcination, only gives out its heat and light slowly and imperceptibly, unless when this operation is performed very rapidly, in combustion gives out that heat quickly and sensibly. Thus the doctrine is, that, by applying heat to a combustible body, we so far overcome the attraction of cohesion as to make the particles enter into a union with those of the gas, which gives out its latent heat and light, thus causing the flame that marks and distinguishes the process. Calcination, too, may be produced so quickly, that the process is attended with red heat, and even with flame. Iron burns with a bright whitish and sometimes a bluish flame, gold with a duller and more lambent flame of a greenish colour.

The product of the combustion, slow or quick, was next attentively considered by M. Lavoisier. In the case of metals it was their calces, or as he denominated them from the process of oxygenation, *oxides*. In the case of sulphur he had found it to be vitriolic acid; in the case of phosphorus phosphoric; nitrous gas, which he erroneously supposed the base of nitrous acid, formed that acid by its union with oxygen. The nature of fixed air, too, was no longer a matter of doubt. Dr. Black had shown, as early as 1757,

* In his Memoir on Phlogiston in the volume for 1783, he speaks of his theory of combustion as having been "published in 1777." If by "published" he means read at the Academy, this may be correct, for it appears to have been read 5 Sept., 1777, but the volume was not published till 1780. In the same volume we find internal evidence that the other papers referred to in the text were read in the opening of that year; thus, one of them read in May refers to experiments about to be performed in company with M. Trudaine and M. Montigny, the former of whom died in August, 1777.

that the combustion of charcoal produced it. M. Lavoisier, in 1777, satisfied himself by his experiments on pyrophorus formed by heating alum and carbonaceous matter together, that the union of carbonaceous matter with oxygen gas produces fixed air. It is true he did not complete this important inference till 1781, when he showed by decisive experiments that charcoal contains, beside inflammable air, water, and other impurities, a matter purely carbonaceous, and which he afterwards termed *carbon*, which, by its union with oxygen, forms fixed air, thence called by him *carbonic acid*. But the knowledge that the something contained in charcoal uniting itself with oxygen gas forms fixed air, and that this fixed air is an acid, had been obtained by Dr. Black, M. Lavoisier, and M. Macquer before 1777. On these facts he now reasoned as well as on the composition of the acid of sugar, which, with other vegetable acids, he considered as containing oxygen. He then made his famous generalization that oxygen is the acidifying principle, and from thence he gave it the name. Dr. Priestley had shown its absorption by the lungs in respiration; and thus we had the general proposition established, as M. Lavoisier supposed, that oxygen gas is necessary to combustion, calcination, acidification, respiration, possibly to the animal heat thence arising, and certainly to the red colour of arterial blood; consequently he held that all those processes, so different in themselves, are really one and the same, the union of oxygen with different bodies in different ways. I reserve for a subsequent stage of the inquiry the consideration of this important and beautiful theory.

While M. Lavoisier was employed in generalizing the phenomena observed by others, in correcting former opinions, and in adding materially to the store of facts by his own experiments, but rather filling up blanks left by his predecessors than producing any very striking novelties himself, two most important disco-

veries were made in England which call for our careful observation,—the composition of water and of the nitrous acid. Respecting the latter discovery there is no question whatever. Mr. Cavendish alone is its author. Dr. Priestley had shown that nitrous acid was resolvable into nitrous gas, which he discovered, and oxygen. M. Lavoisier had never gone further than to suppose that gas the base of the acid. He had never suspected it to be compounded of any other known materials. except in so far as it plainly contained oxygen; and as for azote, the residue of atmospheric air after the oxygen gas, or respirable part, is withdrawn from it, we find him expressing strongly ('Mém.,' 1777,) that this is a body of whose nature we are wholly ignorant. I am not aware that he ever laid any claim whatever to share in Mr. Cavendish's great discovery, to which he was led by the most philosophical consideration of the acid always found when oxygen gas, impure from the presence of nitrogen or azote, is burnt with inflammable air. A careful course of experiments devised and directed by him, performed by his colleagues of the Royal Society, led to the knowledge of this important truth.

But the other great discovery with which his name is inseparably connected stands in different circumstances. Nothing can interfere with his title to be regarded as having first made the capital experiment upon which it rests; but it is equally undeniable, that from less elaborate experiments Mr. Watt had before him drawn the inference then so startling, that it required all the boldness of the philosophic character to venture upon it—the inference that water was not a simple element, but a combination of oxygen with inflammable air, thence called hydrogen gas. That Mr. Watt first generalized the facts so as to arrive at this great truth, I think, has been proved as clearly as any position in the history of physical science. ('Life of Watt,'—Historical note in Appendix.—Eloge of Watt

by Arago.) It is equally certain from the examination of Mr. Cavendish's papers, and from the publication lately made of his journals, first, that he never so clearly as Mr. Watt drew the inference from his experiments; and, secondly, that though those experiments were made before Mr. Watt's inferences, yet Mr. Cavendish's conclusion was not drawn even privately by himself, till after Mr. Watt's inference had been made known to many others.*

In 1783, after Mr. Cavendish's experiment had been made, and after Mr. Watt's theory had been formed upon the experiments of Warltire and Priestley, and of Mr. Watt himself, Sir Charles Blagden went to Paris. The experiments of Mr. Cavendish were made in 1781; the theory of Mr. Watt was contained in a letter which was communicated to the Royal Society in April, 1783: there is even reason to think from his correspondence, that it was formed earlier. Mr. Cavendish never gave the least intimation of having drawn any such inference from his experiment before April, 1783, when Mr. Watt's letter was in the hands of the President of the Royal Society, and was accessible to Sir Charles Blagden, one of the Council. Mr. Cavendish's Diary of his experiments has been carefully examined, and fac-similes have been printed by Mr. Harcourt of all that relates to the discovery; not a word is to be found of the inference or conclusion from the experiment, of a date prior to April, 1783, when Mr. Watt's letter was in the hands of the Society. It is certain that, whether he took the theory from

* Mr. Harcourt's publication, contrary indeed to his design, has, in a very remarkable manner, strengthened the evidence in Mr. Watt's favour. ('Life of Watt,' in vol. i., p. 201.) Professor Robison's article in the 'Encyclopædia Britannica' gives an opinion coinciding with mine; and it was published thirteen years before Mr. Cavendish's death. I first stated that opinion in a published form in 1803-4. ('Edinburgh Review,' vol. iii., p. 11.) See the Appendix to this Life, in which some account is given of the extraordinary errors and carelessness about facts, which distinguish M. Cuvier's Eloge of Mr. Cavendish.

Mr. Watt or had formed it himself, he did, previous to June, 1783, adopt and express the opinion that his experiment showed "dephlogisticated air to be water deprived of its phlogiston." Now this was, in the language of the Stahl doctrine, holding that water was formed by the union of phlogiston with dephlogisticated air,—was a calx, as it were of phlogiston. But Mr. Watt's theory was, that phlogiston and inflammable air are synonymous. Be this, however, as it may, the conclusion contains the real doctrine of the composition of water, how much disguised soever by the language of the phlogistic theory; and that conclusion was communicated, Sir C. Blagden says, "in summer, 1783," to M. Lavoisier. His words are, "that he gave last summer (1783) some account of Mr. Cavendish's experiments to M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of its phlogiston: but at that time so far was M. Lavoisier from thinking any such opinion warranted, that till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water."*

This passage is in Mr. Cavendish's paper; but it is not in his own hand-writing, nor is it in the paper as at first printed; it is added in the hand-writing of Sir C. Blagden, and is therefore that gentleman's assertion of what had passed at Paris the summer before. M. Lavoisier states that it was in June Sir C. Blagden saw him; and also states that he was present when the experiment on which the French claim to the discovery rests, was performed by Messrs. Lavoisier and Laplace before several Academicians on the 24th of June. He adds the material fact, that Sir Charles informed the

* In a letter of Blagden's, published in 'Crell's Annals,' in 1786, he states having mentioned to Lavoisier also Mr. Watt's conclusions, which he there admits had been drawn "about the same time" as Cavendish's. Vol. I. for 1786.

company of Mr. Cavendish's having already performed the experiment, and obtained a considerable quantity of water from the combustion of the two gases. He wholly omits the still more material fact, that Sir Charles also stated the conclusion drawn from the experiment in England; and he omits to mention that he, M. Lavoisier, did not believe it possible that nearly the whole of the two airs could be converted into water. This omission of M. Lavoisier is quite unworthy of him. Sir C. Blagden's statement was published in 1784 in the 'Philosophical Transactions;' and though M. Lavoisier constantly wrote papers which were published by the Academy for several years after this statement of Sir Charles in Mr. Cavendish's paper, and though his Memoirs repeatedly touched upon the composition of water, and in one of them he gave it as a truth established by himself, ('Mém. sur la Décomposition de l'Eau par la Végétation des Plantes,' 1786,) yet he never gave a word of contradiction to Sir C. Blagden's statement. Indeed, that Sir Charles must, if he related the experiment as M. Lavoisier says he did, have also added the conclusion drawn from it, is quite evident; he never could have given the one without the other. If the unbelief of M. Lavoisier was not a fact, it was a pure invention of Sir Charles, which not only M. Lavoisier, but M. Laplace, M. Leroy, and others, all present at the time, could at once have contradicted. And here the reader cannot fail to recollect, that a very similar circumstance attended Dr. Priestley's communication of his discovery of oxygen to M. Lavoisier. When the Doctor described the effect of this new gas in enlarging the flame of bodies burnt in it, M. Lavoisier expressed his great surprise; yet he afterwards suppressed all mention of his surprise, and of his having received the account of the discovery from the real author. In the case of Mr. Cavendish's experiment, he admits having been told of it; and suppresses all mention of the theory having been at the same

time imparted to him, and of his own incredulity until he repeated the experiment and convinced himself.

It seems, therefore, quite certain, that in this case, as in that of oxygen, M. Lavoisier's intrusion is clearly proved; that he performed an experiment which another had before, to his knowledge, contrived and made; that he drew a conclusion from it, in substance the same with the conclusion which others had drawn, and which he had been apprized of, before he either produced the experiment or reasoned upon its results; that he related the whole, both in his 'Memoirs,' and in his 'Elements,' as if he had been the author of the discovery; and that he only told a part of the communication previously made to him, leaving out if he did not suppress, the most important portion of the statement, the theory of the process.

It is on the other hand certain, that from having abandoned the phlogiston hypothesis, his theory of the experiment was more distinctly and accurately given than it had been by former reasoners who were hampered with the errors of that doctrine: although in the popular language at the time, the composition and decomposition of water was always spoken of as the discovery that had been made. We must further allow, that M. Lavoisier added a valuable experiment to the synthetical process of Priestley and Cavendish, the analysis of water by passing its vapour or steam over hot iron filings, and finding that the oxygen calcined the metal, while the other constituent part escaped in the form of inflammable air; an experiment of excellent use after the more crucial trial of the composition had been made, but wholly inconclusive had it stood by itself.*

* An admirable experiment similar to Mr. Cavendish's was performed in June, 1783, by M. Monge, at Mézières. The account of it is given in the volume for 1783; and the author mentions in a note both Lavoisier and Cavendish's experiments, stating that they were performed on a smaller scale.

In the course of these inquiries, of the numerous Memoirs to which they give rise, and of the various discussions in which they involved him, M. Lavoisier, who was so anxious, as we have seen, to obtain a share as a kind of partner in the greatest discoveries of his time, never showed any anxiety to distribute the praise where it was really due, either among his contemporaries or their immediate predecessors. It might have been thought difficult to write so often as he has done upon the gases, and the new æra which their discovery opened to chemistry, and not to have once mentioned him, who, by the discovery of fixed air, was beyond all doubt the founder of the system. Still more difficult was it to investigate the properties of that body, ascertaining its composition with new accuracy, and yet avoid all allusion to Black, who had long before him proved it to be the product of charcoal when burnt. The reader will search in vain, either the papers on combustion, or those on acidification, or those on the composition of fixed air, for the least reference to that illustrious name. In the several Memoirs upon the nature of heat, its absorption and evolution, its combining in a quiescent state to form the permanently elastic fluids, how difficult was it to avoid all mention of him who made the great step of discovering latent heat, and showed that to its absorption was owing fluidity, both liquid and aëriform! I confess that when I first read the title of one of those excellent papers, "De la Combinaison de la Matière du Feu avec les Fluides évaporables, et de la Formation des Fluides élastiques aëriformes," (Mém. de l'Acad. 1777, p. 410,) I expected to find mentioned, at every step of the discussion, the author of this whole theory, and who left it absolutely perfect, who taught it from the year 1763 to crowded classes, and whose name was connected with it wherever science was cultivated. My wonder was not small when I found not the least allusion to Black, and that the problem was completely solved,

how to frame an exact account of any given man's discoveries and theory, never coming into contact with his name. No reader of that paper could doubt that the whole doctrine was that of M. Lavoisier himself; and in a paper printed seven years after by himself and M. de La Place, on the nature of heat, a reference is distinctly made to this doctrine of aëriform fluidity, as the theory of M. Lavoisier.* We find this in the Memoirs for 1780,† published 1784, but the paper was read June 18, 1783. The theory of latent heat had been taught by Dr. Black to large classes for above twenty years before that time, and had been universally associated with his name in every part of the world.

But it may be supposed, that by some singular chance, M. Lavoisier was unacquainted with that illustrious name. I must therefore produce evidence to the contrary under his own hand. In Oct., 1789, he writes to Dr. Black, and professes himself to be "zélé admirateur de la profondeur de votre génie, et des importantes révolutions que vos découvertes ont occasionnées dans la chimie." In the following year, July 14, he tells him; "Accoutumé à vous regarder comme mon maître, je ne serai content jusqu'à ce que les circonstances permettent de vous aller porter moi-même le témoignage de mon admiration, et de me ranger au nombre de vos disciples." Now after writing these letters, M. Lavoisier published his 'Elements;' and while writing them he published, in the Memoirs of the Academy, a paper in which the doctrine of latent heat, as the cause of fluidity, is described, and described as his own, not as Black's, whose name is wholly avoided."‡

* Mem. 1780, p. 399.

† See, too, vol. for 1777, p. 595. In the paper 1777 first cited, the only thing ascribed to preceding philosophers is the belief in the existence of an igneous fluid, or matter of heat in our planet; and the experiments of Richman, Cullen, Mairan, and Baumé on the production of cold by evaporation.

‡ Mem. 1789, p. 567. Black is mentioned with Boyle, Hales, and

It may easily be believed that Dr. Black's surprise was great upon this occasion, and that he treated the flattery contained in these letters with a very marked contempt. This we learn from his friend and colleague, Professor Robison, (Lectures, vol. II., note.) But this no one could have learnt from that illustrious philosopher's manner, when he had occasion to speak of his correspondent in public. I well remember the uniform respect with which he mentioned him in his Lectures, the admiration which he always expressed of his great powers of generalization, the satisfaction with which he recounted his experiments, some of which he himself performed before us; nay, the willingness with which he admitted him to a share of the grand discovery of the composition of water; and showed us the analytical proof, or rather illustration of the doctrine, as a most happy confirmation of it, though not certainly deserving to be regarded as an unequivocal demonstration. No one could ever have suspected either the existence of the letters which I have cited or the blank in the Memoirs with which I have contrasted them.*

After the year 1784, though M. Lavoisier continued his scientific labours, excepting his co-operation in forming the new nomenclature, and his important researches, in company with M. Seguin, upon the processes of respiration and transpiration, there are no results of his chemical inquiries that require to be mentioned. The paper on Respiration (Mem. 1789) contains some very important experiments which throw great light upon that process, and some upon the production of animal heat. They not only clearly show

Priestley, only as having shown that the air of the atmosphere is altered by the respiration of animals.—(p. 568.)

* Other authors of eminent name have been more just, especially Fourcroy, who gives Dr. Black the entire credit of his great discoveries. ('Elem. de Chym.' i., 36, 40; 'Syst. de Con. Chym.' ii., 28, 49.) In one passage he distinctly states these discoveries as having led to the new system. ('Elem.' i., 40.)

that the oxygenation of the blood, in passing through the lungs, produces both carbonic acid gas by the slow combustion of carbon, and water by that of hydrogen, the carbon and the hydrogen being alike supplied by the blood, which as early as 1785 M. Lavoisier had suspected from many appearances;* but they enable us to ascertain the exact quantity of oxygen gas consumed, and of carbon and hydrogen inhaled in the process; for they show 24 cubic feet of gas, or 2 lbs. 1 oz. and 1 scruple to be consumed in 24 hours, and 2 lbs. 5 oz. and 4 scruples of carbonic acid to be formed with 5 scruples 51 gr. of water: answering to 10 oz. 4 scruples of carbon and 1 oz. 5 scruples and 51 gr. of hydrogen. A number of valuable physiological and therapeutical conclusions are derived from the same inquiry. In the paper on Transpiration (Mem. 1790) the inquiry is continued, and a general estimate is formed by approximation of the amount lost in the 24 hours by this process; it is 1 lb. 14 oz. and by respiration only 5 drachms: a calculation not reconcileable with the former course of experiments, which made the loss under 12 oz.

Beside these Memoirs, and one or two others of less importance on chemical subjects, he gave a paper in 1789 upon the horizontal strata deposited by the sea; a subject to which he had, in the earliest period of his scientific researches, devoted much of his attention, as I have already related. From Lavoisier's numerous observations, both on the coast and on the Paris basin, M. Monge drew the conclusion that the earth was originally covered with vegetables long before any animals were upon its surface. The subsequent inquiries, we may say discoveries, of Cuvier and his

* The theory of the present day departs somewhat from Lavoisier's, particularly in holding that the carbonic acid is not produced at the surface of the lungs, and that the oxygen enters into combination with the mass of the blood, forming water and carbonic acid at the capillary terminations of the vessels.

successors, deprive these comparatively imperfect attempts in geological science of nearly their whole interest.

In the course of the illustrious career which we have been surveying, its brightness occasionally dimmed with the spots which a regard for the truth of history overcoming our regard for his fame made it a duty to mark, this great man occasionally gave his aid to the administration of public affairs, not as a politician, for from that craft he ever kept aloof, but when called in by the government to its assistance. In 1776 M. Turgot, then minister, requested him to superintend the manufacture of gunpowder; and the result of his labours was both the increase by nearly a fourth in the explosive force of the compound, and what the enlightened statesman who employed him valued still more, the suppression of the vexatious regulations for collecting saltpetre from private buildings: an operation of wise as well as humane legislation, by which the produce of that necessary article was increased fourfold. When the National Assembly, in 1791, appointed a committee to improve the system of taxation, he was again consulted, and he drew up a treatise, entitled '*Richesse Territoriale de la France.*' which contained the fullest account yet given of the production and consumption of the country, and was by far the most valuable report ever presented to the legislature. Being appointed one of the Commissioners of the Treasury in the same year, he introduced into that great department such system and such regularity, that the income and expenditure under each head could be perceived at a single glance each successive day. To the new metrical system he contributed by accurate experiments upon the expansion of metals, never before fully investigated. He was likewise consulted, with great advantage to the public service, upon the best means of preventing forgery, when the system of paper credit led to the issue of assignats.

The Academy, as well as the state at large, benefited amply by his mature and practical genius, formed to direct and further the affairs of life as well as the speculations of the closet. All its plans, and all the subjects referred to it by the government received the inestimable advantage of his assistance and advice; he was a member of the Board of Consultation, and he was the treasurer of the body, in which capacity he introduced new order and exact economy into the management of its concerns.

These public cares did not distract him from that due to the administration of his private concerns. Agriculture had early in life engaged his attention; and he set apart a considerable tract of land on his estate, at Vendôme, for experimental farming. Of the peasantry upon his property he always took the most kind and parental care; and to the poor, in general, his charities knew no bounds but those of his means. His house in Paris is described as having been a vast laboratory, in which experiments were always going on: not merely those contrived by himself and subservient to his own speculations, but whatever trials any one connected with science desired to have made, and which required the aid of his costly apparatus to perform. Twice a-week his apartments were thrown open to receive scientific men, foreigners as well as natives; all were received with the utmost courtesy; and to young men of merit in straitened circumstances this enlightened and truly liberal person was a generous auxiliary.

The lustre which his labours had shed over the scientific renown of France, the valuable services which he had rendered to her in so many important departments of her affairs, the virtues which adorned his character and made his philosophy beloved as well as revered, were all destined to meet the reward with which the tyranny of vulgar faction is sure to recompense the good and the wise, as often as the base un-

lettered multitude are permitted to bear sway and to place in the seat of dominion their idols, who dupe to betray and finally punish them. The execrable triumvirate in 1794 seized him with twenty-seven others, who had been fermiers-general before the Revolution, an employment he held as it were by inheritance; they were all flung into prison upon a charge which as against most of them, certainly as against Lavoisier, was ridiculously groundless, that of having mixed water and ingredients hurtful to the health of the citizens for the adulteration of tobacco, one of the objects of the ferme; but their real crimes were their possessions. On hearing of the order for his arrest he fled, and remained for some days in concealment; but understanding that his escape might injure the others, and that among them M. Paulzé, his father-in-law, had been arrested, he nobly, though to the sorrow of the sciences, gave himself up and was confined with the rest. He presently perceived that he must expect to be stripped of his property; but he could lead the life of a philosopher, and wealth had never ministered to any but his philosophical pursuits. He had, indeed, when those dismal times began, in conversation with Laborde, said that he foresaw his fortune could not escape, and that he was resolved, when ruined, to support himself by his labour; and the profession in which he designed to engage was that of pharmacy. No such respite, however, was now allowed him. By a retrospective law, monstrous even in that season of violence, their persons were declared punishable for the profits which they had made from the old government, and punishable not as for malversation but treason. This iniquitous decree was passed on the 5th May; under it he was condemned to death by the Revolutionary Tribunal, before whom a courageous citizen, M. Hallé, had the noble firmness to read a detailed account of Lavoisier's discoveries, and his services to his country. After his sentence was pronounced, he himself asked to be

allowed a few days' respite, in order that he might see the result of some experiments which he had planned, and which were going on during his confinement; the cruel answer of the Tribunal, through Coffinhal their brutal jester, was that "the Republic had no need of philosophers," and he was hurried to the scaffold on the following day, the 8th of May, 1794, with a hundred and twenty-three other victims, who suffered in the course of a few hours.

Thus perished, in the fifty-first year of his age, one of the most illustrious cultivators of science in modern times. When the absolutely harmless life he had ever led, remote from all political connections, is considered, together with the utterly ridiculous nature of the charge against him, we can hardly avoid asking ourselves how it came to pass that no voice was raised, no hand stretched out for his rescue. One man of science, among the most eminent of his time—Carnot, was on the Terrible Committee: had he no means of saving this great philosopher, accused of something as absurd and fabulous as witchcraft? There was another, much more nearly related to Lavoisier in his pursuits—a member possessed of no small influence in the Convention, and who had in the Committee of Public Instruction succeeded in carrying some most important measures—Fourcroy was that man; and he had often employed his extraordinary powers in explaining and enforcing the great discoveries of his master, as well as in sounding his praises to crowded audiences assembled from every part of the world. Fourcroy could never have feared to receive the answer of the savage, Coffinhal, that the Republic had philosophers enough; and it is to be hoped that Fourcroy did not consider there would be philosophers enough if his master were to disappear from among their number. The courage shown by the virtuous Hallé might have been expected from Fourcroy, in whom its display would have been incomparably safer. His interposition would also have

been much more powerful; nay, we know that he did interpose, with effect, for another member of the Academy, M. Darcet, whom he saved from the guillotine. No explanation has ever been given of the neutral position maintained by him in Lavoisier's apparent murder. This only we know, that he remained in his place, both as a member of the Convention and of the Committee; and we know, too, how impossible it would have been to retain Halley or Maclaurin in their place, had the sacred head of Newton been threatened by the sacrilegious hands of their colleagues. The charge against Fourcroy amounts to no more; for there is no evidence whatever to support the accusation often brought against him, that he had instigated the atrocious crime which placed all the republic of letters in mourning, and covered that of France with infamy hardly to be effaced. M. Cuvier tells us that the "most strict researches had left him unable to discover the least proof in support of this horrid charge," and he states that this imputation "had been the torment of M. Fourcroy's life."* This is very credible; the charge is hardly credible at all. But men's admiration of Hallé will remain for ever; and if their suspicions of Fourcroy should ever be removed, they must at least regard his want of courage with contempt rather than pity.

The great man thus sacrificed, was as much to be loved in private life as he was to be revered among philosophers. His manners were simple and engaging, his generosity unbounded, his conduct without reproach. His case formed no exception to the general rule, which seems almost always to forbid genius from descending in families, for he died childless. His widow, a person of remarkable abilities and great information, shared in his pursuits, and even took upon herself the task of engraving the plates that accom-

* Eloge de Fourcroy, Mém. de l'Institut, An. 1810. (Tome XI., Phys. et Math.)

panied his 'Elements.' She survived him many years, and late in life was married to Count Rumford, whom she also outlived.

From the accurate detail into which I have entered of Lavoisier's history, no difficulty remains in forming an estimate of his merits as a great teacher of science. He possessed the happiest powers of generalizing, and of applying them to the facts which others had discovered, often making important additions to those facts; always, where any link was wanting to connect them, either together or with his conclusions, supplying that link by judiciously-contrived experiments of his own. He may most justly be said to have made some of the most important discoveries in modern times, and to have left the science of chemistry with its bounds extended very far beyond those within which he had found it confined when his researches began.

It is, however, fit that we make the important distinction between the two classes of his theories: those which being founded upon a rigorous induction, and not pushed beyond the legitimate conclusions from certain facts, stand as truths to this day, and in all probability will ever retain their place; and those which, carried incautiously or daringly beyond the proper bounds of him who is only *naturæ minister et interpres*,* have already been overthrown—never, indeed, having reposed upon solid foundations.

1. Of the first class is his important doctrine of calcination—justly termed by him, oxidation,—by which he overthrew the leading doctrine of Stahl, and showed that metals do not part with anything in passing from the reguline state, but, on the contrary, absorb and fix a gas—proved by other philosophers to be oxygen gas. This, his capital discovery, stands, and in all probability will ever stand, the test of every inquiry.

* Bacon.

We know of no calcination without oxygen—we know of no metallic oxygenation without calcination.*

2. The importance of the blow thus given to the theory of phlogiston induced him to follow it up by denying that combustion is a process which evolves any component part from bodies; but, on the contrary, that, like calcination, it always consists of some other substance being added to, or united with, the inflammable body.

3. The ascertaining the nature of fixed air, that is, the combination of oxygen gas with the carbonic principle, and the ascertaining also the existence of that principle, is another discovery of the same great master; and we owe it to the well-contrived experiments by which he proved it.

4. The analogy of the diamond to this carbonic principle is another discovery of his, though he did not make the final step of showing the identity of the two bodies, nor did he even suspect it.

5. The composition of sulphuric and of phosphoric acid, and perhaps of saccharic too, were first clearly explained by his experiments, and by his judicious and original reasoning upon the experiments of others.

6. There is more doubt of the composition of the atmosphere having been first proved by him. Certainly its nature was by him first fully ascertained; but it was plainly known to Priestley at an earlier date. Lavoisier, however, added much to our accurate knowledge of the function of respiration; and the discovery of hydrogen being evolved by it as well as carbon, was undeniably his.

7. We have seen that to the two great discoveries of oxygen and the composition of water, he can lay no claim. Yet let it be borne in mind that his statement of both doctrines was more precise and clear than any

* If it should be said that metals absorb oxygen when dissolved in oxygenous acids, we answer, that still they are in the state of calx or oxide, though united to an acid menstruum.

which the authors of the experiments and original framers of the theory had given. As regards the latter doctrine, the obscurity of Mr. Cavendish's language, even of Mr. Watt's though in a much less degree, has been observed upon already. But we need only consider Dr. Priestley's view of the air he had discovered, and the name he gave it, in order to be satisfied how confused were the notions derived from the phlogistic theory, and how they obscured his naturally acute vision. When he called it dephlogisticated air, he intended to say that air, the atmosphere, parts with phlogiston, and the residue is oxygen gas. But then if phlogiston be added, it should again become common air. Now he held the calcination of metals to be the evolution of phlogiston, therefore this operation should have restored the gas to the state of common air. But, instead of that, the gas was absorbed altogether. Again: the residue, when common air is deprived of the dephlogisticated portion, is another air which he called phlogisticated, because it contained more phlogiston than the common air. But how by this theory could the union of such a phlogisticated air with a dephlogisticated air make the common air? By the hypothesis, that air with phlogiston added is azote, with phlogiston subtracted is oxygen gas. Therefore mixing the two, you should have produced, not the air that had been phlogisticated in making the one, dephlogisticated in making the other, but double the quantity operated upon.* Such was the load of absurdity and contradiction under which the favourite hypothesis of the day placed Priestley entirely, Cavendish to a great degree, Watt in some sort; such was the weight of prejudice against which Lavoisier had to contend; such was the maze of error from which he boldly broke loose and extricated chemical science. It is his glory that he first effected this emancipation;

* If common air (a) — Phlog. = ox. gas, and com. air (a) + Phlog. = azote; Ox. gas + azote = not a, as it ought to be, but 2 a.

and it is no small proof of his merit, that for many years he remained almost alone among the philosophers of his age, and even his own countrymen, how prone soever to adopt French discoveries, in maintaining opinions from which there is now, after the lapse of little more than half a century, not a single dissenting voice all over the scientific world.

We are now to mark wherein he was led astray by the love of theorising carrying him too far. He was not content with showing that combustion, contrary to the phlogistic doctrine, proceeds from a union of the burning body with other bodies; but he regarded the body uniting as always the same, to wit, oxygen. Observing the fact of many bodies burning in oxygen gas, and of most other gases being unfit for supporting flame, he generalized too much, and inferred that all combustion consists in the union of that gas with the inflammable body.—Again: he regarded the heat and light given out in the process as wholly proceeding from the gas, as having kept the gas when latent in its aëriform state, and as given out in a sensible form when the gas becomes fixed in a liquid or a solid state.—Lastly: observing that the union of many bodies with oxygen produced acids, he generalized too much this fact, and inferred that all acids contain oxygen, which he thence called by that name, as denoting the acidifying principle. Now all these inferences are groundless, and therefore this portion of his theory is to be rejected. He is to be followed implicitly in rejecting Stahl's principle; the doctrine of phlogiston he for ever overthrew. His own theory, the doctrine which he substituted in place of the one which he had destroyed, is liable to insuperable objections; at least when carried to the length which he went.

In the *first* place, not only may oxygenation take place without any evolution of either heat or light, but combustion also. The mixture of many substances together evolves heat, and a great degree of heat, with-

out the presence of oxygen—or if oxygen be present in some of these cases, it is not operative in any way—it is not disengaged, and is not in the form of a gas to be absorbed. Thus, much heat is caused by the mixture of sulphuric acid and water; some heat by the mixture of alcohol and water. Lime when slaked by water produces violent heat, sometimes accompanied with light also, flame as well as redness appearing. The union of iron with sulphur in vacuo causes great heat and the emission of bright light. The exposure of metals and other inflammable bodies to gases which contain no oxygen, as chlorine, produces red heat and flame. Therefore, although it is very true that we know of no instance in which combustion takes place without the union of the combustible body to some other, and the formation of a new substance, yet it is not true that oxygen alone causes combustion, and that no body can burn but in oxygen gas.

Secondly. The facts are all against his doctrine, that the heat and light comes from the fixation of the gas. Experiments on the capacity of bodies for heat have clearly shown this. But the simple fact of well-known explosions, as of gunpowder, disproves his theory—for here, instead of the heat and light coming from the gas being reduced to a solid state, a gaseous body is formed two or three hundred times the bulk of the solid exploded.

Thirdly. There are many acids which have no oxygen in their composition, and there are many bodies containing oxygen which have none of the qualities of acids. The first part of this proposition was not certainly known to Lavoisier, and he assumed that the acids which had not yet been decomposed would be found to contain oxygen. The second part of the proposition was known to him, and ought to have checked his generalization. We now know many acids which contain no oxygen at all. Muriatic acid, a compound of chlorine and hydrogen; prussic acid, a

compound of hydrogen, nitrogen, and carbon; hydrobromic; fluoric-acid; ferro-cyanic acid; sulpho-cyanic; hydro-selenic; hydriodic: xanthic. Even if fluoric be omitted, here are nine undeniable acids, and all without a particle of oxygen in their composition. Again, the mere fact of calcination should have prevented him from so generalizing, for all calces contain oxygen, and many of them have no acid qualities. Indeed, his own conjecture, since fully confirmed by experiment, that the fixed alkalis are oxides, is a still more striking disproof of his theory; for it appears that he might just as well have called oxygen the alkalizing principle as the acidifying, or rather much better, since all the alkalis save one contain it and the alkaline earths to boot. But he also should have recollected that no acid of them all contains so much oxygen as water, and yet nothing less like an acid can well be imagined. We now have still further instances of the same kind against this theory, and which might justify us in calling hydrogen the acidifying principle as well as oxygen. Upwards of two hundred acids contain hydrogen either with or without oxygen present. Hence he might really have reckoned hydrogen the acidifying principle upon fully better grounds than support his choice of oxygen; and the truth appears to be, that there is no one substance which deserves the name.

It is, then, quite clear that M. Lavoisier committed a great error in his induction, and that he framed a theory which was in the extent to which he pressed it wholly without foundation—not merely without sufficient proof from the facts, but contrary to the facts. Newton gives it as a fundamental rule of philosophising, that we are to state the inferences from phenomena with the exceptions which occur, and if a first induction should be made from imperfect views of the phenomena, then to correct it by the exceptions afterwards found to exist. But from this rule Lavoisier has

departed entirely: because, though subsequent experiments have greatly increased the number of the exceptions, yet there were many striking ones at the time he formed his system, and these were left out of view in its formation.

After all the deductions, however, which can fairly be made from his merits, these stand high indeed, and leave his renown as brilliant as that of any one who has ever cultivated physical science. The overthrow of the Phlogiston Theory, and the happy generalizations upon the combinations of bodies, which we owe to his genius for philosophical research, are sufficient to place him among the first, perhaps to make him be regarded as the first reformer of chemical science, the principal founder of that magnificent fabric which now fills so ample a space in the eye of every student of nature.

APPENDIX.

Acids known to contain no Oxygen.

Muriatic acid, (Hydro-chloric; Chlorine and Hydrogen.)
Prussic acid, (Hydro-cyanic; Hydrogen, Nitrogen, and Carbon.)
Bromine.
Hydro-Bromic acid, (Bromine and Hydrogen.)
Fluoric acid, (Fluorine and Hydrogen.)
Ferro-cyanic acid, (Iron, Azote, Carbon and Hydrogen.)
Sulpho-cyanic, (Sulphur, Azote, Carbon and Hydrogen.)
Hydriodic, (Iodine and Hydrogen.)
Hydro-selenic, (Selenium and Hydrogen.)

Acids known to contain Hydrogen with or without Oxygen.

Muriatic, (or Hydro-chloric.)	Formic.	Acetic.
	Oleic.	Tartaric.
Prussic, (Hydro-cyanic.)	Stearic.	Citric.
	Capric.	Malic.
Hydro-bromic.	Butyric.	Benzoic.
Hydro-fluoric.	Crotonic.	Gallic.
Hydriodic.	Racemic.	Succinic.
Hydro-selenic.	Cetic.	Saccholactic.
Ferro-cyanic.	Cholesteric.	
Sulpho-cyanic.	Ambreic.	

And at least 150 more; as oxalic is perhaps the only vegetable acid which has no hydrogen.

SIR JOSEPH BANKS.

It is rare to observe a name among the active and successful promoters of science, and which yet cannot easily find a place in its annals from the circumstance of not being inscribed on any work, or connected with any remarkable discovery. Almost all the philosophers of both ancient and modern times have left us writings in which their doctrines were delivered, and the steps made by their labours were recorded. The illustrious exception of Socrates almost ceases to be one, from the memory of his opinions being preserved by two of his disciples in their immortal works; and the important discoveries of Archimedes and of Pythagoras are known distinctly enough in the books of ancient geometry, to leave no doubt resting upon their claims to the admiration and the gratitude of all ages. The lost works of the ancient geometers evidently afford no exception to the general remark, since they once existed, and contained the discoveries of their authors.

It must, however, be observed, that the circumstance of a cultivator of science having left no works to after ages is merely accidental. He may have enriched philosophy with his achievements, and yet never have recorded them himself. Thus, had Black only made the great discovery of latent heat and specific heat, he would have been justly considered in all times as one of the greatest benefactors of natural science, and yet the history of that splendid discovery would only have been found in the memory of those who had heard his lectures; his only work being confined to the other discovery of fixed air, and the nature of the alkaline earths. To

take a yet more remarkable instance;—how little of Watt's great and lasting fame depends on any written work which he has left! The like may be truly said of Arkwright; nay, the most important of inventions, the art of printing, is disputed by two names, Coster and Guttenberg, neither of which is connected with the composition of any literary work whatever.

As men who have by their researches advanced the bounds of science,—"inventas aut qui vitam excoluere per artes,"—may never have given any written works to the world, and yet merit a high place among the greatest philosophers, so may others who have filled the less exalted but highly useful sphere of furthering the progress of the sciences or the arts, deserve a distinguished place among philosophers for the same reason which entitles authors to such a station, although they may never have contributed by any discoveries to the advancement of the sciences which they cultivated. The excellent and eminent individual whose life we are about to contemplate falls within this description; for although his active exertions for upwards of half a century left traces most deeply marked in the history of the natural sciences, and though his whole life was given up to their pursuit, it so happened, that with the exception of one or two tracts upon agricultural and horticultural questions, he never gave any work of his own composition to the world, nor left behind him anything, beyond his extensive correspondence with other cultivators of science. It is from this circumstance that not even an attempt has ever been made to write the history of Sir Joseph Banks. And yet, what so worthy of contemplation as the history of one who loved science for its own sake, who delighted in the survey of important facts connected with the study of nature, or tracing interesting truths belonging to the same branch of knowledge; whose pursuit of knowledge was wholly disinterested, not even stimulated by the hope of fame as the reward of his labours?

And who better deserved the name of a philosopher than he whose life was devoted to the love of wisdom, whose rich reward was the delight of the study, whose more noble ambition left to others the gratification of recording their progress in books, and filling the mouths of men with their names? Much of what is explained, touching the real pleasures of science, in the life of D'Alembert, is applicable to the career of Sir Joseph Banks.*

He was of an ancient and wealthy family, established since the reign of Edward III., first in the West Riding of Yorkshire, and afterwards in the county of Lincoln, where they possessed ample estates from the end of the seventeenth century; and a considerable accession of fortune came to them early in the eighteenth, by marriage with an heiress in Derbyshire, named Hodgkinson, whose estates, by a shifting use in a settlement, were severed from those in Lincolnshire till 1792, when the whole fortune united in the person of Sir Joseph.

He was born at Argyle Buildings, in London, on the 2d of February, 1743, O. S., according to a note in his own handwriting which lies before me, contrary to several accounts which represent him as born in Lincolnshire in December of that year.† After being placed for some time under a private tutor, he was in his ninth year sent to Harrow and four years after to Eton, where his good disposition and cheerful temper recommended him to his masters; but they complained of his extreme aversion to study, and inordinate love of active sports. In about twelve months, however, when in his fourteenth year, his tutor found him reading at the hours of play, and the change which had been effected in his habits was described by himself to Sir Everard Home as arising from an accidental circumstance. One day he had been bathing with his fellow Etonians; and on coming out of the water to dress,

* See Life of D'Alembert, and Appendix to Life of Robertson, vol. ii.

† The parish register of St. James's makes his birth 4th January.

he found that all but himself had gone away. Having put on his clothes, he walked slowly along a green lane. It was a fine summer's evening; flowers covered the sides of the path. He felt delighted with the natural beauties around him, and exclaimed, "How beautiful! Would it not be far more reasonable to make me learn the nature of these plants than the Greek and Latin I am confined to?" His next reflection was that he must do his duty, obey his father's commands, and reconcile himself to the learning of the school. But this did not hinder him from immediately applying to the study of botany; and having no better instructor, he paid some women who were employed in gathering plants—what is called culling simples—for the druggists, for such information as they could give him, the price he gave being sixpence for each thing they told him. Returning home for the holidays, he was inexpressibly delighted to find in his mother's dressing-room an old torn copy of Gerard's Herbal, having the names and figures of those plants with which he had formed an imperfect acquaintance, and he carried it with him back to school. There he continued his collection of plants, and he also made one of butterflies and other insects. I have often heard my father say, that being of the same age, they used to associate much together. Both were fond of walking and of swimming, and both were expert in the latter exercise. Banks always distinguished him, and in his old age he never ceased to show me every kindness in his power, in consequence of this old connection. My father described him as a remarkably fine-looking, strong, and active boy, whom no fatigue could subdue, and no peril daunt; and his whole time out of school was given up to hunting after plants and insects, making a *hortus siccus* of the one, and forming a cabinet of the other. As often as Banks could induce him to quit his task in reading or in verse-making, he would take him on his long rambles; and I suppose it

was from this early taste that we had at Brougham so many butterflies, beetles, and other insects, as well as a cabinet of shells and fossils. The interesting anecdote related by Sir E. Home, I never heard my father relate, but he always said that his friend Joe cared mighty little for his book, and could not well understand any one taking to Greek and Latin. The anecdote itself must be perfectly authentic if Sir E. Home heard it from him; for he was scrupulously exact in relating facts, and anything like romance about natural scenery was the thing in the world the most alien from the cast of his mind.

In 1760 he was taken from Eton to be inoculated, and the operation failed: it was repeated, and succeeded; but so much time was thus lost, that it was thought better he should not return to school; and immediately before he completed his eighteenth year, he was sent to Oxford, and entered a gentleman commoner of Christ Church. His love of natural history now increased with the increased means and greater leisure for gratifying it. Botany, however, continued to be his favourite branch of that science; and he found that unfortunately no lectures were given by Dr. Sibthorp, the botanical professor. In this difficulty he applied to the learned doctor for leave to engage a lecturer, whose remuneration should be wholly defrayed by his pupils; and it is highly creditable to the professor, and shows his love of the science, in which some of his family afterwards so greatly excelled, that he at once agreed to the proposal. Mr. Banks then finding no one at Oxford capable of undertaking the class, went over to Cambridge, whence he brought back with him Mr. Israel Lyon, a learned botanist, and good astronomer, who was then engaged in teaching these two sciences to private pupils. The friendship of Mr. Banks afterwards obtained for him the appointment of astronomer to Captain Phipps on his Polar voyage. Mr. Lyon gave lectures and lessons to

the young men who had joined in this very laudable scheme, and Mr. Banks, as might be expected, profited exceedingly by those instructions. Among true Oxonians, of course, he stood low. He used to tell in after-life, that when he entered any of the rooms where discussions on classical points were going briskly on, they would say, "Here is Banks, but he knows nothing of Greek." He made no reply, but he would say to himself, "I shall very soon beat you all in a kind of knowledge I think infinitely more important;" and it happened that, soon after he first heard these jokes, as often as the classical men were puzzled on a point of natural history, they said, "We must go to Banks."

In 1761 his father died; and in 1764, on coming of age, he was put in possession of his valuable estates in Lincolnshire, having quitted Oxford the year before. And now it was that the great merit of this distinguished person shone forth. With all the incitements which his age, his figure, and his station naturally presented to leading a life of idleness, varied only by the more vulgar gratifications of sense or of ordinary ambition, and with a fortune which placed these gratifications in ample measure within his reach, he continued steadily devoted to scientific pursuits, and only lived for the studies of the naturalist. He remained out of Parliament; he went little into any society but that of learned men; his relaxation was confined to exercise, and to angling, of which he was so fond, that he would devote days and even nights to it; and as it happened that Lord Sandwich had the same taste, and that both possessed estates in Lincolnshire, they became intimately acquainted, and saw much of one another. So zealous were both these friends in the prosecution of this sport, that Sir Joseph used to tell of a project they had formed for suddenly draining the Serpentine by letting off the water; and he was wont to lament their scheme being discovered the night

before it was to have been executed: their hope was to have thrown much light on the state and habits of the fish.

In May, 1766, he was elected a member of the Royal Society, and the same year he accompanied his friend Sir Thomas Adams in the *Niger*, entrusted with a voyage to Newfoundland. Mr. Banks's object was the collection of plants: what the object of the particular voyage might be I am not informed. On his return to England by way of Lisbon, early in 1767, he resumed, or rather continued, his studies in botany and natural history; and the intimacy which he formed with Dr. Solander, a favourite pupil of Linnæus, now settled at the British Museum as Assistant-Librarian, greatly facilitated his application to these pursuits.

The commencement of George the Third's reign was distinguished most honourably, both for the Sovereign and for his favourite minister, Lord Bute, by an extraordinary regard for the interests of science. That distinguished person, the victim of much popular prejudice and misrepresentation, formed a rare exception to most statesmen who have governed this country, for he was fond of philosophical studies, and was a successful as well as a diligent cultivator of some of the sciences. Accordingly, the patronage of the Crown was extended to others who had like tastes, and it was most judiciously employed in promoting the discovery of distant regions not before explored by the adventurous spirit of navigators. Captain Wallis had recently brought us acquainted with some of the more remarkable groups of islands which stud one portion of the Pacific Ocean; and it was resolved to promote these discoveries, for the advancement of natural science, without any views of conquest. In 1676 Halley, while residing at the Island of St. Helena, had made an important observation on the transit of Mercury over the sun's disc. But he had bequeathed to astronomers a far more important recommendation, to mark the transit of Venus, an event

of much more rare occurrence, and which he could not hope to see, as it was calculated to happen next in the year 1761. He had shown how complete a measure that phenomenon would afford of the sun's parallax, or the angle subtended by the earth's radius at the surface of the sun. This angle could be with great accuracy best ascertained by different contemporaneous observations at distant points of the arc which the planet described in its passage,—the planet affording, as it were, an object between the sun and the earth, a kind of signal-post, by means of which the angle sought might be measured.

Accordingly, in 1761 the British Government sent one observer, Mr. Mason, to the Cape, and another Dr. Maskelyne, to St. Helena. The French Government at the same time sent Le Gentil to Pondicherry, in the East Indies, and La Chappe to Tobolsk, in Siberia, and Pingre to Rodrigues, near the Mauritius. But the weather proved so unfavourable that no certain conclusion could be derived from their observations; for though Pingre and Mason's observations proved afterwards to be correct, they differed so widely from the others that the whole subject remained in great uncertainty. A second transit was expected in 1769, and the British Government now sent an astronomer (Mr. Green) again to make those important observations.

The great value of the object in view is manifest. If we can ascertain the parallax, we have, by an easy process, the exact distance of the sun from the earth; for, as in every triangle the sides are as the sines of the opposite angles, the distance of the sun must be to the earth's radius as the sine of an angle not sensibly differing from a right angle, that is, as unity to the sine of the horizontal parallax. Hence the distance is equal to the radius of the earth divided by the sine of that very small angle. The distances from the sun of the other planets are easily found, because we know their relative distances; and the real diameters of the sun

and of these bodies are likewise deducible from the same angles. The whole structure of the planetary or solar system thus depended upon ascertaining the angle of parallax; and nothing, therefore, could be more becoming the rulers of two such kingdoms as France and England, than to promote by every means the success of these observations. While one expedition was sent to the Pacific, Otaheite being the place chosen for the experiment, Messrs. Dymond and Wales repaired to Hudson's Bay, Mr. Call to Madras, and the Abbé de la Chappe was sent to California. The Danish Government sent Father Hell to Wardhus, near the North Cape: the King of Sweden dispatched Planmann to Caianeborg in Finland; and the Empress of Russia sent several observers to different parts of Siberia, with the same views. Four of the observers—those at Otaheite, California, Hudson's Bay, and Wardhus—were completely successful. The expedition to the Pacific had for its principal, but not its only object, the observation by Mr. Green of the transit. Everything that regarded the natural history of the island fell within its scope; and the accurate survey of the coasts already known, as well as the exploring of new lands, was an important part of the wise and enlightened scheme.

As soon as Mr. Banks found that the voyage to the South Seas was resolved upon, he applied to his friend Lord Sandwich, then at the head of the Admiralty, for leave to join the expedition with a suite of scientific men, and this was immediately granted. He made his preparations on the most liberal and extensive scale, worthy of his fortune and his zeal for the advancement of natural knowledge. He took with him Dr. Solander, the distinguished botanist already mentioned. He likewise took two draftsmen and four servants; and, as the expedition was placed under the government of the naval service, all who joined it became subject to its rules and its discipline.

The choice of Captain Cook, as commander, was

singularly fortunate, or rather it was perfectly judicious. He had risen gradually from the humble station of an apprentice in a collier of Whitby, till he became mate of a vessel engaged in that trade, fitted beyond all others to make excellent navigators, because it is carried on by sailing upon a coast almost without any harbour of refuge, and consequently exposes the mariner to constant risks and exercises his unremitting vigilance. When the war of 1756 broke out, (the Seven Years' War,) he had volunteered into the navy, and showed such talents in his profession, that the Admiralty appointed him mate of a sloop, the *Mersey*, in which he was present at the siege of Quebec, under Wolfe. His skill and gallantry in laying down the river and its soundings, previous to the attack, led to his being employed in making a chart of the St. Lawrence as far as the sea. Though he had never been taught either surveying or drawing, this chart was long the only one in use. He was, in consequence, made master of the *Northumberland* frigate, and served in that capacity till 1762, employing, however, his spare time in the study of the mathematics, in which he received most valuable assistance from a person of great science, a pupil of the Bernouillis, Mr. afterwards Major Desbarres; and in 1764, his patron, Sir Hugh Palliser, whose name has been blackened by the assiduous efforts of political faction, but who for many years was the firm friend and only patron of Cook, being appointed to the Government of Newfoundland, obtained for him the place of marine surveyor of that island and Labrador. He held this place for nearly four years, and enriched hydrographical science by the most valuable charts of those regions. The talents which he had displayed as a navigator were united to every bodily quality that can fit men for either action, or labour, or suffering—an eye sure in estimating directions and distances; a frame of iron; an entire indifference to fatigue, or privations, or the times of wakefulness or of rest.

But these natural aptitudes for great actions were even exceeded by his excellent demeanour in every station whether of obedience or of command, by his fertility of resources in all difficult situations, by his calmness in danger, his firmness and presence of mind on every emergency, "Plurimum audaciæ ad pericula capessenda, plurimum consilii inter ipsa pericula erat; nullo labore aut corpus fatigari aut animus vinci poterat. Caloris ac frigoris patientia par; cibi potionisque desiderio naturali, non voluptate, modus finitus. Vigiliarum somnique nec die nec nocte discriminata tempora: id quod gerendis rebus superesset quieti datum." (Liv. xxi. c. 4.)

So accomplished a seaman, or one so admirably fitted for exploring new and unknown regions, guided only by science and relying only on his own resources in all perils and all emergencies, has never perhaps been offered to the choice of a Government desirous of promoting this interesting and difficult branch of the public service. He was accordingly promoted to the rank of Lieutenant and placed at the head of this expedition. Such was the chief under whom Mr. Banks embarked in this important enterprise; and in admiration of his great qualities he yielded to none of his followers. There was, indeed, something exceedingly congenial in the two characters; the same love of discipline, the same firmness of purpose, the same exclusive devotion to the one object in view, the same strict and even punctilious regard to the performance of his duty, the same active habits, and the same contempt of everything but action, distinguished alike these eminent individuals, and knit them together in an indissoluble friendship notwithstanding the somewhat stern temper of the one and the occasionally irascible disposition of the other, and notwithstanding the wide difference of the favourite pursuits to which their several lives had been devoted. There was, moreover, a considerable difference of age; for Banks

was only in his twenty-sixth year, while Cook was upwards of forty.

On the 25th of August, 1768, the *Endeavour* sailed from Plymouth Sound; but the jealousy of the Brazil Government preventing them from landing at Rio de Janeiro, the first land at which they touched, (except a few days at Madeira,) was the Terra del Fuego, the southernmost point of the great American continent. Here Mr. Banks and Dr. Solander made extensive botanical collections; but though it was the height of summer in that severe climate, their attempts to ascend the mountains were attended with extreme danger from the severity of the snow storms and the excessive cold. Three of their attendants perished; and Dr. Solander could only be saved from that deep sleep which proves the forerunner of death, by the greater activity and more powerful constitution of his younger companion, who succeeded himself in casting off the drowsiness by a strong and painful effort, and was enabled also to rescue his friend. I have more than once heard him discourse on the subject: he described the desire of sleep which then stole over his senses as altogether irresistible, and ascribed its force to the effect of the cold in making all other desires with all the faculties torpid. Motion seemed to produce little effect, for the irresistible tendency was at every step to sink down, as if the greatest suffering was to continue alive and awake, the most delightful state to fall asleep and expire; nor, so far as I recollect his account, did any of them, while yielding to this propensity, doubt that it was indulged at the cost of life itself. Dr. Solander's case was peculiarly remarkable. Accustomed to excessive cold in travelling among the Norwegian and Swedish Alps, he had warned his companions of the fate that awaited them should they yield to drowsiness. "Whoever," said he, "sits down, will sleep; whoever sleeps will wake no more." Yet was he soonest overpowered. He insisted on being suffered to lie down. One of the

men said, "all he desired was to lay down and die." The Doctor did not quite say so; but he acted on this feeling. He fell asleep before he could reach the fire which Mr. Banks had kindled. When the latter roused him, his feet were found to be so shrunk that his shoes fell off.

On the 26th of January, 1769, they sailed from Cape Horn, and arrived, after a prosperous voyage, at Otaheite, on the 11th of April. The delightful climate, pleasing landscape, and amiable people which here met them, may well be supposed to have enchanted men who for eight long months had seen only the sea and the sky, unless when they touched on the arid and inhospitable coast of Terra del Fuego. But amid their repose and relaxation, business never was forgotten. They spent the time that elapsed before the Transit in astronomical observations, and in a minute examination of the island. Mr. Banks and his friend became thoroughly acquainted with every branch of its natural history, but he also acquired extraordinary favour and influence with the natives, insomuch that he became the frequent arbiter in their disputes. This ascendant he owed to his frank and manly carriage, his perfect good humour, and his unfailing firmness, to which we must certainly add his noble presence, so well fitted to make an impressiou upon rude minds. An important service was rendered by him, and he was enabled to render it through the influence which he had thus acquired. When the observatory was established on the 1st of May, and the instruments had been taken on shore the evening before, it was found that the quadrant, contained in a large packing-case, and deposited in a tent guarded by a sentinel, had been carried off. The whole object of the expedition was frustrated should it not be recovered. Every search proved unavailing. At last Mr. Banks went into the woods, and his judicious and spirited exertions proved successful; the precious instrument was restored

in perfect safety. In his expedition he was sometimes surrounded by the crowd of impatient and angry natives, and had to show his pistols in order to control them. He went among them with a single attendant only.

The event so anxiously expected, of the Transit, took place at the time prefixed by the calculations,—June 3. As the critical day approached, the general anxiety increased, and it descended from the astronomer himself to the humblest mariner of the expedition. On the night of the 2nd not an eye was closed. One rose every half hour to report the state of the weather to the rest, who were kept on the alert by the hope which arose when the sky was reported clear, or the fear which the mention of a cloud produced; but next morning, to their unspeakable delight, the sun was seen to rise without a cloud, and the serene clear sky continued during the day. The observations were accordingly among the best of any which the different astronomers made of the phenomenon. The precaution had been taken, judiciously suggested by Lord Moreton, of making the observation at more places than one; and Mr. Banks accompanied the party which was despatched for that purpose to the Island of Eimeo. An officer was sent to another station on the main island, while Captain Cook and Dr. Solander remained at the fort erected at Otaheite, with Mr. Green, who there found the first external contact to be at 9, 25, 42, and the beginning of emersion, and the total emersion 3, 32, 10; so that about six hours of serene and clear weather were required for this important observation. The latitude was 17° 29′ 15″ south,—the longitude 149° 32′ 30″ west.

In the same year the transit of Mercury was afterwards observed with equal success in the island of Major near Mowtohera on the 9th November. The weather, though it had been very thick for several days before, proved most propitious on the 9th. Mr.

Green made the internal contact 12, 8, 58, the external 12, 9, 55. Captain Cook's observation differed one second as to the former, seven as to the latter. The latitude was 36° 48′ 28″ south.

From the observations of the transit of Venus by the expedition compared with the four others in Siberia, Lapland, Hudson's Bay, and California, the sun's parallax was determined at 8″·78, and his distance from the earth was thence deduced to be 93,726,900 miles, upon the supposition that the radius of the earth is 3985. The relative distances of the planets being known, those of them all from the sun were then determined.*

About six weeks after this important transaction, the *Endeavour* proceeded on her voyage; and first the navigators cruised for some time among the group, then little known, of the Society Islands. They next proceeded in search of the great Southern Continent, the Terra Australis, so long supposed to exist as a balance to the lands of the Northern hemisphere. On the 9th of October it was thought to be discovered, land being on that morning seen, with mountains of a lofty height; but it proved to be New Zealand, discovered, in 1620 by Tasman, who called it Staaten Island, but never landed upon it; nor had it ever been since visited. Captain Cook during six months sailed round it, and fully explored its coasts. He found it to consist of two large islands. On the 31st March, 1770, he began his homeward voyage, and directed his course along the east coast of New Holland, never before explored, and indeed then quite unknown. On this voyage every opportunity was seized of extending our knowledge, both of the natural history and the geography of that vast region. The navigation was most perilous, because the coast is surrounded with sharp

* Mercury, 36,281,700; Venus, 67,795,500; Mars, 142,218,000; Jupiter, 487,472,000, and Saturn, 894,162,000.—See 'Phil. Trans.' vol. lx., 1574, Prof. Hawley's paper.

coral reefs, which rise suddenly like a wall from the water.

In spite of all difficulties he had safely run along about 1300 miles of this unknown and savage coast, when on the night of the 10th of June, some hours after an alarm of being on a coral reef had been felt, but passed away, a loud crash, followed quickly by a second, too plainly told them that the vessel had struck. The commander was instantly upon deck. I have heard Sir Joseph Banks describe his habit of nightly making all the arrangements, and giving all the orders which he deemed necessary when running along an unknown coast, and having a lee-shore under his bow. After the usual direction to call him if anything occurred, he would then calmly undress and go to bed, satisfied that all precautions had been taken for every event which could be foreseen or conjectured, and he was immediately asleep. Upon that trying occasion he was upon deck in his drawers as the second blow was struck, and he gave his orders with his wonted coolness and precision. The ship had grounded on a coral reef, which surrounded her almost to the surface of the water, but in a calm sea made no breach, and could not be seen. She had been carried by the waves clear over the ledge of rock, and lay on a hollow within it, in some parts of which the water was not more than three or four feet deep. The light of the moon showed, to complete their distress, the sheathing-boards of the ship floating all around, and at last her false keel, so that their fate appeared imminent. It was necessary to lighten her by all means, though the probability appeared slight of her holding together till another tide should enable them to get her off. The morning disclosed a full view of their dreadful and dismal condition. The land was at eight leagues' distance, and no islets lay in the intermediate sea, on which the crew could be landed and saved were they to quit the wreck, the boats being wholly insufficient to take all the crew

at once. Nothing could possibly be more desperate than this appearance of things. Nevertheless, the sense of imminent danger produced the strictest discipline; no attempts at insubordination were perceivable; nor any discontent; but rather an alacrity, approaching to cheerfulness, was shown by all; and it was observed that their awful situation restrained any loose or profane expressions, so that not an oath was to be heard any more than a murmur. To lighten the ship, was now the first object. Every thing, therefore, was thrown overboard which could be spared, guns, heavy lumber, ballast, stores; and yet two tides elapsed before she could be got afloat. The moment of her floating was truly an anxious one; for the water had gained so fast that there was a great probability of her going down when no longer supported by the rocks. Every one saw in his neighbour's countenance a reflection of the despair he felt himself; but none gave way to such feelings, and the suspense continued in silent anxiety and dread. To their exceeding relief, at ten in the morning, when she rode in deep water, the leak was found to gain no faster than before, though her bottom was by that time considerably damaged. The water, however, could only be stemmed by the unceasing labour of the crew at the pumps night and day. The men were so exhausted, that finding the leak still gain upon them, they were on the point of giving it up in despair, when one of the midshipmen suggested the having recourse to an expedient which he had seen practised on a voyage to America, called *fothering*. It consists in drawing under the ship's bottom a sail in which there are stitched down oakum, flax, dung, and other thick and light substances. The motion of the leak draws in the sail with its stuff, and thus stops or lessens the leak. He represented this process as having proved so successful when he saw it tried, that the vessel was allowed to make her homeward voyage without further repair. Happily, being now tried, it

succeeded to a wish, and enabled a single pump to keep the leak under.

They proceeded on their voyage till a river was discovered in which they could give the ship (whose name it now bears), the necessary repairs. But upon laying her down and examining her bottom, they found to what a singular circumstance they owed their providential escape. A large fragment of the coral had forced its way through the timber, and was found sticking in the leak so as in a great measure to stop it, otherwise the size of the aperture was such that it must have sent the vessel at once to the bottom. The boats being wholly insufficient to save the crew, it may easily be conceived with what feelings all regarded this most extraordinary escape. Captain Cook, in his account of the voyage, gives high praise to all, (he mentions Mr. Banks and his party expressly), for their cool and orderly conduct, and their firm and active exertions during this perilous crisis.

A new calamity, however, now appeared to sadden them, when the joy had scarcely subsided to which their merciful escape gave rise. The scurvy began to make its appearance; and, among others, Mr. Green the astronomer, and Tupia, a native who had accompanied them from the wish to visit England, were so severely attacked that there seemed no means of stemming the disease. The country was explored to find fresh vegetables for the relief of the sick, and Mr. Banks, with his wonted activity and skill, served to guide these important expeditions. In the course of them he discovered the strange quadruped since so familiarly known both to naturalists and the vulgar, the kangaroo. He also found a supply of fish, turtle, and large cockles, and some vegetables, which proved a most seasonable relief. Nor were his researches concerning the manners and habits of the natives less interesting to science; indeed, it is principally to him that we owe the accurate descriptions of the natives seen and conversed

with in the course of the voyage, a description which forms a new and important chapter in the general history of our species. In prosecuting these inquiries his courage was as conspicuous as his activity and his judgment. He would expose himself to their collected multitudes when some inadvertent proceeding had roused their anger, or would resist them when a thirst of plunder incited them to threaten; he would visit their habitations unattended by any force whatever; he would sleep for nights together on the ground at many leagues' distance from the crew of the vessel, and accompanied only by two or three attendants, regardless of the peril in which he must have been placed had the natives, possibly living close by, discovered the place of his repose.

After remaining on this coast above six weeks, they set sail again on the 3rd of August, but it was a grievous disappointment to find, on examining the pumps, that they were all decayed and unfit for service, so that their only trust was in the strength of the vessel's timbers. Fortunately she made no more than an inch of water in the hour. A gale, which soon assailed them, and lasted for some days, did no material damage. The navigation was, however, beset by reefs of rocks and shoals, through the narrow openings of which they escaped almost miraculously. At length, after three months of constant peril, they burst as it were into a wide and deep sea, the swell of which showed that no land was near. The leak, however, had now increased to nine inches an hour, and in two days more they were surrounded by breakers, and in a more dangerous position than ever: nor did they escape except by the sudden springing up of a light breeze at the moment when they were helplessly and hopelessly drifting on the rocks.

Then, after repairing the vessel, Captain Cook proceeded on his cruise through the most intricate navigation in the world; then, too, first explored the tract

of reefs and islands on the northern part of the east coast; and having now examined and laid down above two thousand miles of coast, he formally took possession of the country for the British crown, giving it the name of New South Wales. From thence he proceeded to New Guinea, which he proved to be an island separate from New Holland; and, on the 9th of October he arrived at Batavia, where it was necessary to give the ship a thorough repair. Upon examining her bottom it was found in many places worn to the thinness of the sole of a shoe, and in other places it appeared that there had, since the accident, been nothing between them and the water but a lock of wool jammed between the planks; so small was the distance, so feeble the barrier by which, in traversing the Indian Ocean, they had been for weeks separated and saved from the unfathomable deep! The gravest malady, however, that visited the expedition, the fever, now broke out in that pestilential climate. Seven of the crew died in a few days; and so many more were sick that not ten men remained fit for duty. Mr. Banks and Dr. Solander were so ill that their lives were despaired of, and they were only saved by going into the country. The iron frame of Cook himself was seen to yield; he, too, fell seriously ill. When they set sail, 26th December, Mr. Banks was carried on board, and his life still despaired of. The ravages of the fever continued throughout the voyage; and the nightly corse was frequently heard to plunge in the water. Before they reached the Cape, 15th March, three-and-twenty thus perished, including Mr. Green the astronomer, and Mr. Munkhouse, the midshipman, whose suggestion had saved the ship. After remaining there a month, they sailed for England. Nothing material occurred on the voyage, and on the 12th July, 1771, the *Endeavour* cast anchor in the Downs, giving over her gallant and prudent commander, with

his adventurous company, to the admiration and gratitude of their country.

Before the vessel was allowed to have any communication with the shore, Captain Cook required every person on Board to deliver up all his journals, notes, drawings and other papers—a requisition which was immediately and cheerfully complied with. No leave was given to make any disclosures or any separate publication until the Government had determined on the person into whose hands the official accounts should be placed for being communicated to the public. Dr. Hawkesworth was pitched upon, and he is allowed to have performed his task with reasonable ability and with perfect fidelity. Mr. Parkinson, brother of one of Sir Joseph Banks's attendants, indeed his draughtsman, broke through the rule, and published a tract with drawings; but the book was speedily bought up by his liberal and spirited employer, and the irregular publication proceeded no further.

The results of the voyage were highly important. The observations necessary for ascertaining the solar parallax had been made with perfect success. The manners of the natives in the Society Islands had been examined, and the singular state of their society ascertained. Their products, vegetable, mineral, and animal, as well as those of New Holland, New Zealand, and New Guinea, had been fully explored, chiefly by Mr. Banks and his learned companion. The coast of New Holland had been thoroughly surveyed as well as the whole of New Zealand. These two islands had been shown not to form a portion of any southern continent; and the existence of such a continent as far as the 47th degree of south latitude had been disproved. All now joined in rendering due praise to the leaders of the expedition; and its illustrious commander was immediately raised a step in the naval profession. But it is fit that we here pause to reflect on the large share which Mr. Banks had in the conduct of the expedition, that

is, in the collection of the vast and important information which was its result—information not confined to natural history, but extending to the manners, the habits, and the condition of the natives. It was from the record duly and faithfully kept of his observations that the history of the voyage was subsequently compiled; and Dr. Hawkesworth (*Introduction*) expressly states that he felt concerned at delivering his account in the person of the commander, when, as to all but the nautical part, he would have preferred making Mr. Banks speak. This was proposed to him, "but the proposal was generously overruled!"

Important, however, as were the results of the voyage, it had not extended our knowledge of the southern hemisphere beyond the 47th degree; and as it was still supposed possible that the Terra Australis might be in a higher latitude, to which the instructions of Capt. Cook had not before reached, a new expedition was fitted out early in the following year, under the same great navigator. It is impossible to reflect without astonishment and admiration on that ardour for the advancement of science, and that noble disregard of both dangers and fatigues, and annoyance of every kind, in the pursuit of his favourite object which could induce Mr. Banks again, after a few months of repose, to volunteer his services. These were gladly accepted, and his preparations were made on so vast a scale as required, even with his ample means, the raising of money by way of loan. He engaged Zoffany, the painter, and three draughtsmen; he took two secretaries and nine servants, well versed in the art of preserving plants and animals; all the books, drawings, and instruments required for his studies, and all the stores which so numerous a suite could desire, were provided with profusion. Everything seemed ready for his joining the expedition, when the constant thwarting which he received at each turn from the Navy Board, especially from its chief, the Comptroller, wore out his

patience, and he reluctantly abandoned this enterprise so near his heart. The name of the wrong-doer must not be suffered to perish, and thus escape the scorn which it so well deserves from each friend of science, and of a liberal and enlightened national policy; nor must it be concealed even because of the great service he had before rendered by his patronage of Capt. Cook. The Comptroller who thus thwarted both the wishes of the scientific world and the views of his own official superiors, probably from being one that

> Hated learning worse than toad or asp,

was Sir Hugh Palliser. The common report that Capt. Cook had himself objected to and frustrated Sir Joseph's plan of accompanying him, is against all probability, and it rests on no evidence whatever. A letter of the Captain's is given in the Appendix, and it betokens an entire disposition to aid his friend and fellow-voyager in his arrangements.

Mr. Banks, however, was determined not altogether to lose the fruits of his extensive and costly preparations for an expedition which he was thus prevented from joining. He fitted out a voyage to Iceland, which he undertook with his trusty and tried friend, Dr. Solander, and with a Swedish clergyman, Dr. Von Troil, of Iceland. Including draughtsmen, secretaries, seamen and attendants, there were forty persons in company; and in August, 1772, they reached the island. They remained there for a month, examining everything that related to its natural history, and especially the volcano, Hecla, and the boiling springs, Reykum and Geyser, for which it is famous. A rich collection of books and manuscripts was likewise purchased, and presented by Mr. Banks to the British Museum. Dr. Von Troil, who afterwards became Archbishop of Upsal, published a full and interesting account of the voyage. Mr. Banks left the subject in his hands with his wonted aversion to the pursuit of literary fame, and his habitual

undervaluing of all but the exertions required to perform great or useful actions.

After his return to England, he settled in London, except the short period every summer which he passed at Revesby, his seat in Lincolnshire. His hospitality in the country was quite unbounded, and extended to all parties and all classes. His house in Soho Square was with its noble library, and precious collection of maps, drawings, and engravings, connected with botany, and the various branches of natural history, always open to the student and the author. Foreigners as well as natives were ever his welcome guests, and it was his delight to be surrounded by the cultivators and the promoters of science in all its branches.

In 1777, Sir John Pringle resigned his office as President of the Royal Society, and in Nov. 1778, Mr. Banks was chosen his successor. He lost no time in devoting himself with his accustomed ardour to the duties of his high station, and for some years his administration was carried on with general approval. But the person who undertakes to reform abuses in any public department, must lay his account with making enemies; and though these may be at the first few in number, and of little weight, they form a centre, around which will soon gather all, who on any account are discontented; all who regard a superior with envy or an exalted equal with jealousy; not seldom all who would fain displace him, and succeed to his station. So it fared with Mr. Banks; for he too, had early perceived, and speedily checked some manifest abuses. The chief of these was the ease with which the door of the Society was opened, to admit all who desired to be Fellows. The Secretaries might be almost said to elect them at their pleasure; for whoever wished to add the title of F.R.S. to his name, as author of a book, or as a Divine seeking preferment, or as a Physician in quest of practice, had only to become acquainted with those officers, and obtain their good will. Their con-

stant intercourse with the members gave them so many opportunities of recommendation, that the election was quite secure of any whom they chose to favour. The President was little consulted, whose especial duty, however, it is, to preserve the purity of election, and to see that improper or improvident choice be not made. It is well known that D'Alembert, in allusion to the extreme prodigality with which the honours of the fellowship were distributed, was in use to ask jocularly any person going to England, if he desired to be made a member, as he could easily obtain it for him, should he think it any honour. The new President was resolved that this should no longer be allowed, and though the unlimited number of its fellows must always prevent the place of F.R.S. from being an object of so much value and of so much desire as that of an Academician in France, he thought that at all events it should be restored to somewhat of its primitive value, by being no longer indiscriminately bestowed. Two principles were laid down by him; *first*, that any person who had successfully cultivated science, especially by original investigations, should be admitted, whatever might be his rank or his fortune; *secondly*, that men of wealth or station, disposed to promote, adorn, and patronise science, should, but with due caution and deliberation, be occasionally allowed to enter. There could be no objection to these principles, or to limiting the choice in future to cases thus defined. It is to be lamented that an end was not also put to the extremely absurd and even degrading statute by which, while all others must have their claims published twelve weeks before being considered, Peers and Privy Councillors may be balloted for the moment they are proposed—a law every way bad in itself, and worse in its execution, for that which is really intended as distinction, is in practice regarded as unimportant, and the claim of no person of rank is ever subjected to the least scrutiny; he is chosen at once on being proposed. But

the other and pressing case of abuse, the indiscriminate election, was at once corrected by Mr. Banks, and with a firm hand. He announced to the secretaries and members his determination to watch over the applications for admission, and the election by ballot. Previous to the election, he spoke to the members who usually attended; he gave his opinion freely on the merits of candidates, and when he considered a rejection proper, he hesitated not to advise it—giving his opinion, and recommending, or asking a black-ball from individuals at the time of the ballot. The consequence was the rejection of several persons, and this was afterwards made the chief ground of attack upon him in the dissensions which unfortunately broke out, and for some time grievously disturbed the peace of the Society.

The immediate occasion of these dissensions, however, was an accident of a different kind. The office of Secretary for Foreign Correspondence had been conferred upon Dr. Charles Hutton, a mathematician of distinguished reputation, and whose official duties at the Royal Academy of Woolwich, obliged him to reside there. Some neglect of his duties, as Secretary was said to have been thus occasioned. Upon examination, the charge was found to rest on very insufficient grounds; and the childish complaint of M. Bonnet, of Geneva, that a dry and laconic answer had been returned to his letter, accompanying a present of his works, really appears to be the only remains of the accusation which a full inquiry left standing against Dr. Hutton.* It was a much more serious charge, that he held no communication with the President; and certainly this was mainly imputable to his residing at a distance. The Council passed a resolution, 20th

* The feeble and very trimming tract of Dr. Kippis on these disputes, seriously represents the dry style of Dr. Hutton's letter as worthy of blame. The doctor desiderated more courtesy, that is, flummery after the foreign fashion.

November, 1783, recommending that the Foreign Secretary should reside in London, and Dr. Hutton tendered his resignation. The emoluments of the office were only twenty pounds a-year, from a bequest of Mr. Keck half a century before; and Dr. Hutton having to hire chambers in town for the performance of his official duties, had been in reality a loser by holding the place.

This resolution of the Council, and resignation of the Foreign Secretary, immediately caused a great sensation in the society. It appears that the embers of discontent with the President's administration had been for some time smouldering; and now the spark accidentally flung, made them break out in a flame. Dr. Maskelyne, the Astronomer Royal, really considered his friend Dr. Hutton as ill-treated; so might Baron Maseres, and one or two others; but the most active mover, who indeed took the lead in the opposition to the Council, was Dr. Horsley, a priest of intolerant nature, of extreme arrogance, of violent temper, and guided by a most inflated estimate of his own importance as a cultivator of mathematical science, in which capacity he was nearly if not altogether insignificant. Finding himself joined with Dr. Maskelyne and Baron Maseres, he chose to hoist a standard for the mathematical sciences in opposition to natural history, which the President and his especial friends chiefly cultivated; and he considered the treatment of Dr. Hutton as an overt act of hostility to those studies to which he untruly represented that his own life was devoted.

The motion was carried, by a majority of thirty to twenty-five, that Dr. Hutton be thanked for his services as Foreign Secretary; and Sir Joseph Banks's party committed their first error in opposing this proposition, on a ground, plausible, but wholly insufficient, that the Council alone, and not the Society at large, had the means of judging how far the duties of Dr. Hutton's office had been well performed. The New Council

coming into office 29th November, affirmed, with a single dissentient voice, the resolution of their predecessors, requiring the Foreign Secretary to reside in London. Before the next meeting of the Society, Dr. Hutton's written defence was read, and a resolution was passed by a large majority (45 to 15), that, "if he had been censured, he had fully justified himself." Here the matter might have ended, and here it certainly would have ended, had the case of Dr. Hutton alone furnished the matter of dispute. But it was the occasion, not the cause of the dissension. A party had clearly been formed against the President: at the head of that party Dr. Horsley had placed himself; he had raised an absurd cry that the mathematics were neglected, and botany alone patronised; and he was plainly looking to eject Sir Joseph Banks, and raise himself to the chair. This enabled the latter to commit his second great error—the calling in members who were only titular, and never took a part in the ordinary business of the Society, any more than they were capable of sharing in its labours. These came down on the 8th January, 1784, in great numbers; and, after a long debate, they carried, by a large majority of 119 to 42, a vote of confidence, "approving of Sir Joseph Banks as President, and resolving to support him in his office." At a subsequent meeting, a motion for rescinding the resolution of the Council on the residence of the foreign Secretary, was lost by a majority of 85 to 47; as were afterwards, by still larger majorities, three several motions, censuring all endeavours of the President to influence the votes of members by solicitation, either on elections or on any other matters. The two most important of these motions were lost by 115 to 27, the other by 102 to 23. Mr. Maty, a person of some accomplishments, of amiable character, of hasty, fickle temper, who had warmly sided with the President's opponents, soon after resigned his place as one of the ordinary secre-

taries; Dr. Blagden was chosen his successor by 139 to 39, Dr. Hutton being the other candidate. It was possibly a third error of the President, that he sought for a defender in a learned equity barrister, the Accountant-general, Mr. Anguish, who was unknown in the Society for any philosophic attainments; while the opposite party, in availing themselves of Mr. Poore's and Mr. Watson's aid, had advocates who were respected in the literary world.

The main charge urged in these debates against the President, was his interference with elections: and this was loudly objected to, both as overbearing, even despotic, and as having excluded several persons, worthy of the honour they sought. The general objection was wholly groundless. Sir Joseph Banks only interfered as he was bound by the duty of his office to interfere: and if his frank and manly nature, despising all indirect roads to his object, scorning all covert proceedings to attain that which he felt bound to seek—the honour and the advantage of the Society—made him openly state his objections to candidates, and openly ask his brethren to join in rejecting them, instead of canvassing against them in the dark, no better reason can be assigned for loudly applauding the course which he took. That he might have committed mistakes in one or two instances is equally certain. The rejection of Major Desbarres, already mentioned as a pupil of the Bernouillis, and the instructor of Captain Cook, and soon after appointed to a foreign government, was the strongest case cited; the only other person of admitted merit, among the twelve black-balled in four years, was Mr. Henry Clarke of Manchester, a schoolmaster, and a writer of some merit on mathematical subjects; and all admitted that the President's interference had proved most useful to the Society's honour, in carrying the rejection of four or five unworthy candidates. These, under the old and lax system, would in all probability have found

their way into the Society, though their object only was to use the title of Fellows as a snare for enticing customers.*

As for the charge of favouring natural history at the expense of the severer sciences, never was anything more unfounded. Full as many papers had been received and printed by Sir Joseph Banks's Council on the latter subject, as had ever been so treated in any other period; quite as small a proportion of papers upon the former. The Copley medal, five times bestowed, had been thrice given to mathematical and astronomical papers, twice to chemical; and I may add, never either then or since, to papers upon the subjects which the President was supposed most to favour. The appearance of a naturalist in the chair was a phenomenon by no means now for the first time observed in the sphere of the Society. Sir Isaac Newton himself had been succeeded by Sir Hans Sloane, who filled the chair fourteen years, and preceded by Lord Somers, whose eminence is certainly not scientific, though it may be of a higher order. Of the nineteen Presidents before Sir Joseph Banks, nearly, if not quite the greater number were men of eminent station, who never, either before or after their elevation to the chair, were known to have cultivated, much less improved, any branch of "natural knowledge." Nor let it be supposed, as Dr. Horsley and his more factious adherents used to represent, that none but botanists opposed their proceedings, and sided with the President. The names of Cavendish, Watson, Fordyce, Heberden, Hunter, Kirwan, are quite sufficient, both in number and value, to rescue Sir J. Banks's supporters from that imputation, and to take from their adversaries all pretence that they had a monopoly of important science.

Although the majorities were obtained and the de-

* One was the patentee of a new water-closet.

bates chiefly carried on by men who did not usually attend, there can be no doubt that the Society was greatly benefitted by their interference. The asperity which had marked the progress of the contest was testified in the speeches of the opposition leader, Dr. Horsley. He had, moreover, given a great and threatening notice of so many motions as might occupy the Society for the whole session, and until the annual period came round for electing the officers, when he plainly hinted his hope that another President would be chosen. The haughtiness of this arrogant ecclesiastic's tone in the debates gave general offence, even to those who might be disposed to admit the cleverness of his speeches. When, perceiving a defeat approaching, he threatened a secession of the mathematical party, he exclaimed, "The President will then be left with his train of feeble amateurs and that toy* upon the table;—the ghost of the Society in which Philosophy once reigned, and Newton presided as her minister."

To have saved the Society from such a consummation as being under Dr. Horsley's presidency was truly a service of the highest value, which, in a somewhat unusual though certainly not an irregular manner, was rendered by the members who attended and resisted the factious combination. His assuming the station of leader among the mathematicians was altogether preposterous; and he might have been raised to the chair, by dint of the intrigue which he set on foot, and the ferment which he excited in the bosom of the Society, without any victory whatever being gained for mathematical and physical science. His writings had never placed him higher than a mere "amateur," and a somewhat "feeble amateur" in all essentials, though stout enough in the overbearing language of his polemical writings, and magniloquent enough in the diction of his self-laudatory prefaces. Some of his efforts are merely

* The mace, to which he pointed.

puerile, like the Sieve of Eratosthenes, which he tried, he says, "*Diis propitiis usus*;" some are far too easy to confer any fame, like the restoration of Apollonius's Inclinations; while his great attempt, an edition of Newton, is confessed by all to be as signal a failure as any on record in the history of science.*

The escape from such a chief was further enhanced in value by the excellent qualities of him whom the victory kept in the chair. He showed no jealousy of any rival, no prejudice in one person's favour rather than another's. He was equally accessible to all, for counsel and for help; where his own knowledge did not suffice, he could easily obtain the aid of those more devoted to the subject of the application. His house, his library, his whole valuable collections, were at all times open to men of science; while his credit, both with our own and foreign Governments, and, if need were, the resource of his purse, was ever ready to help the prosecution of their inquiries. I know of many persons, since eminent, who when only tyros in science, and wholly unknown to fame, have been patronized by

* The reader who compares Bishop Horsley's praises of his own exploits with the exploits themselves, will readily concur in Professor Playfair's opinion of them expressed delicately but sharply in the fourth volume of the 'Edinburgh Review.' He has not indeed entered into particulars, as to the great failure, the 'Newton.' But who can read an edition of the 'Principia,' the 'Optics,' and the 'Fluxions,' published in 1778-80, and not marvel at the author's apparent ignorance of all that had been done since Sir I. Newton's time? There is not a word of the Calculus of Variations or of Partial Differences,—not an allusion to D'Alembert's principle of Dynamics,—nor to the objection of the Bernouillis and D'Alembert, touching the Hydraulic Cataract;—no reference to the progress of Hydrodynamical science;—nor to the discoveries of Dollond and others on refraction. Indeed the 'Optics' is given almost without note or comment, while the comments on the 'Principia' are only upon passages of no difficulty, leaving the darker ones in their original obscurity, unless where reference is made to the commentary of Le Sueur and Jacquier,—Varignon and Herman and the Bernouillis are unnoticed. In short no one can read the book, however cursorily, and rise from its perusal with the least respect for the Right Reverend Editor, or the least disposition to admit his claim either as head of the mathematicians whom he marshalled to defeat, or as aspiring to fill the Society's chair.

him; and one of these tells me, with grateful recollection, of the kindness he experienced in his younger days from that useful and liberal patron, "who would (says my friend) send all over Europe and further to get either the information or the thing that I wished to have." Where private aid failed of the desired effects, he had access to the Government; he could obtain countenance and assistance from the public departments, beside removing those many and so often insurmountable obstacles which the forms of office and the prejudices of official men plant in the way of literary research.

Many circumstances concurred to give Sir Joseph Banks the power which he so largely exercised of patronizing and promoting the labours of scientific men. His ample fortune; the station which he filled in society; the favour which he enjoyed at Court and with the Ministers of the Crown; the fame of his voyages; his indefatigable industry; his ever-wakeful attention to the representations and requests of the student; his entire freedom from all the meaner feelings which mere literary men are but too apt to entertain one towards another; his great natural quickness and unerring sagacity, never leaving him long to seek for the point of any argument, nor ever suffering him to be deceived by plausible errors or designing parties; his large and accurate knowledge of mankind, and of men as well as of man; the practical wisdom which he had gathered from extensive and varied experience—all formed in him an assemblage of qualities, natural and acquired, extrinsic or accidental, and intrinsic or native, so rare as had hardly ever met together in any other individual.

. . . Quid virtus et quid sapientia possit
Utile proposuit nobis exemplar Ulyssem.
. . . . . Multorum providus urbes
Et mores hominum inspexit; latumque per æquor
Dum sibi, dum sociis reditum parat, aspera multa
Pertulit adversis rerum immersabilis undis.—(*Hor. Ep.*)

He was thus for upwards of forty years the great promoter of philosophical pursuits; and it may fairly be said, that no one, either before or since his time, ever occupied the high station in which he was placed with such eminent advantage to the interests of the scientific world.

His own studies continued, as they always had been, devoted to natural history; and botany was the portion of it which he chiefly loved to cultivate. He was, perhaps, the most accomplished botanist of his day, and among the very first in the other branches of natural history. During the greater part of his life his time and his fortune were assiduously bestowed on the preparation of a magnificent series of botanical drawings and engravings. But he never retained any of these, as it were, locked up for his own gratification; and his habitual indifference to literary fame made him so slow to publish, that he is believed to have given over to other cultivators of the same studies the fruits of his own labour, as constantly as these fruits were ripened and ready to be gathered in; and while all men's books were crowded with his designs, and all men's inquiries promoted by the stores of his knowledge, he alone reaped no fame from his researches, nor profited by the treasures which he had amassed, except by the gratification of seeing them made subservient to the progress of his favourite pursuits.

A baronetcy had been bestowed on him in 1780, and in 1795 he was invested with the Order of the Bath, a rare instance in those days of this distinction being bestowed on any but a military or a diplomatic person; not, however, by any means the first instance; for Sir Robert Atkins, the Chief Baron, was also a Knight of the Order. In 1797, he was made a Privy Councillor. He was chosen Recorder of Boston on the Duke of Ancaster's death. Though often pressed to take a seat in Parliament, he always declined. The favour which he enjoyed with George

III. was of long standing: that Prince loved the manly frankness of his character, the courage with which he had so often exposed himself to danger in the pursuit of knowledge, and the firmness with which his conduct was marked on all trying occasions. Sir Joseph's political principles, too, those of a high tory, were much to the Monarch's liking; and a country gentleman who never troubled himself with Parliamentary life, nor ever desired to rise above the rank he was born to, was sure to find a friend in His Majesty. Though a tory, and very firm in his opinions, both in Church and State, he was anything rather than a party man. He never interfered in politics beyond using his legitimate influence in Lincolnshire and Derbyshire, where his property lay, to aid those country gentlemen whom he believed fitted to make useful representatives of the landed interest; and so entirely devoid of common party feelings was his use of this influence, that he always supported Lord Yarborough, then Mr. Pelham, a whig, as well as Mr. Chaplin, a tory. This just and impartial conduct was not displeasing to the King; and among other marks of good-will, was his recommending to Sir Joseph an attention to agricultural pursuits. I have heard him say that he took to farming by the King's desire. He pursued this pleasing occupation with his characteristic energy, and understood its principles thoroughly, as he practised it with far more than the success that usually attends amateurs. When the King fell hopelessly ill, in 1811, I well remember Sir Joseph Banks saying, he had ceased since then being a farmer, having only "taken up the trade by his Majesty's commands."

A common story is to be found in the slight attempts that have been made to write his life, as if the Ministers were used occasionally to employ his personal influence with the King, to obtain his consent to measures which he disliked. I will venture to give this statement a very peremptory contradiction. I am

pretty confident that he never would have undertaken any such mission; but I am perfectly certain that the King never would have suffered Sir Joseph to approach him on any subject of the kind. This opinion I can state the more emphatically, since my worthy friend Sir E. Knatchbull, who did me the favour of examining this Life, gives me the most positive assurance of his uncle never having at all interfered, as the story asserts he did. An interference of a very different description he did exert, and with the happiest results. During the long war, which desolated the world by land and by sea, after the year 1792, he constantly exerted himself to mitigate its evils, and alleviate its pressure upon men of science and upon the interests of philosophy. It was owing to him that our Government issued orders in favour of La Pérouse, wheresoever our fleets should come in contact with that unfortunate navigator. When D'Entrecasteaux was sent in search of him, and Billardière's collections were captured and brought to England, Sir Joseph Banks had them restored to him, and without even opening to examine them, as if he feared that any one should profit by any discoveries save their rightful owner, the author. On ten several occasions did he procure the restoration to the Jardin des Plantes of collections addressed to that noble establishment, and which had fallen a prey to our naval superiority. He sent to the Cape of Good Hope, to recover some charts belonging to Humboldt, which our cruizers had seized, and in no instance would he suffer the expenses he had undergone to be repaid. He even interfered to remedy injuries which foreign nations had inflicted on scientific men. Broussonet had fled from France to save his life from the anarchists of Paris. Sir Joseph Banks directed his correspondents in Spain and in Portugal to supply his wants; and he found a friendly purse open to him both at Madrid and at Lisbon. Dolomieu, cast into a dungeon in Sicily by the tyranny of the

profligate and cruel Queen, experienced the humanity of Sir Joseph during a long captivity, although his unwearied efforts to obtain his liberation failed of success. His own countrymen, when detained by the arbitrary and perfidious policy of Napoleon, were in repeated instances indebted to Sir Joseph Banks for their permission to return home; and a learned friend of mine, one of the first Oriental scholars of the age, the late Professor Hamilton, must have perished at Verdun but for his generous interference. By his interposition the Institute exerted itself in various other cases; and whenever it could be made to appear that a man of science or of letters was among the detained, no very strict scrutiny being exercised either by Sir Joseph or his Paris colleagues, the order for his liberation was applied for and obtained.

In 1802 he was chosen one of the Foreign Members of that illustrious body, and in acknowledging this high honour he expressed his gratitude in warm terms. Much offence was given to the zealots of the Anti-Gallican party in this country; the remains of Bishop Horsley's party were roused to censure him; an anonymous attack upon him was published in the daily papers, and afterwards acknowledged to have proceeded from the Bishop; Mr. Cobbett, then as bitter an enemy of France and of peace as he soon afterwards became a zealous friend, addressed a letter to the Members of the Royal Society, calling upon them to depose the President from the chair, because he had called the Institute the first literary body in Europe; but the silly faction and the paltry storm it had raised, soon sank into their natural insignificance, and all men of sense saw plainly that nothing in the complimentary language of his letter exceeded the ordinary limits of such compositions, or betokened the least want of respect for his own Society.*

* If Mr. Cobbett was ever less happy on one occasion than another, it was when he meddled with such subjects. He congratulated his country

His assiduous cultivation of natural history, and his devotion to agricultural pursuits, did not prevent him from taking the most active part in promoting the discovery of unknown regions, the most favourite of all his pursuits. He was the real founder of the African Association; and it is well known that when Ledyard, the most accomplished of the travellers next to Mungo Park, was in want of support on his celebrated journey, it was on Sir Joseph Banks that he drew a bill, which in the remote region where the traveller then was, found an immediate honour and discount. The captivity of Flinders, whom I have heard him more than once compare to Cook, was greatly mitigated by his exertions and influence with the French Government; and he not only promoted discovery with all his means to the end of his life, but applied himself vigorously to improving the discoveries successively made to the real use of mankind. The good treatment of the aborigines was ever a main object of his humane exertions. He it was who may be truly said to have planted and founded the colony of Botany Bay. He it was, too, who suggested the means of transplanting the bread-fruit tree from the South Sea Islands to the West Indies, (the object of Captain Bligh's unfortunate voyage,) and of also naturalizing there the mango of Bengal. The fruits of Ceylon and of Persia were successfully, through his exertions and experiments, brought from thence to the West Indies and to Europe. So little did his love of plants end, like that of other botanists, in mere description and classification, in the composition of a catalogue, or the preparation of a Herbal! Horticulture, indeed, was a subject the usefulness of which was sure strongly to attract his care, and accordingly the Society for its improvement owed

in one of his papers on Captain Glennie having discovered the quadrature of the circle,—the captain having gained his scientific fame, in Cobbett's eyes, by joining in the combination against the Duke of York, a year or two before.

its success, if not its origin, to him. The British Museum was a constant object of his anxious care, and during the forty-two years of his official trusteeship he paid unremitting attention to its concerns, and largely endowed it with presents; he bequeathed to it his noble library and all his principal collections.

I have already said that his published works bore no proportion either to his scientific labours or his exertions in behalf of learned men. They consisted only of some tracts on agricultural and horticultural subjects, as the mildew in wheat, and Merino sheep—on Indian and spring wheat—on the Spanish chesnut—on Roman forcing-houses—and some others.

For the last thirty years of his life, Sir Joseph Banks suffered frequently and severely from gout; and during the last fourteen years he was so much a martyr to it, that he could take no exercise on foot. He tried various expedients to lessen the violence of the attacks, such as giving up the use of fermented liquors, and abstaining entirely from animal food; but if the fits were less severe, their recurrence was more frequent. Small doses of Husson's medicine were latterly resorted to with considerable effect; and with his wonted sagacity and firmness he met the objections of those who urged how certain the tendency of that cure was to shorten life, by asking "how many years they supposed he could hope to live if he took none of it?" At last he gradually sank under the exhausting effects of this ailment, after having for a considerable length of time entirely lost the use of his lower limbs. He died at his villa of Spring Grove, Hounslow, 19th June, 1820, in the seventy-eighth year of his age, after suffering with the greatest cheerfulness for many years the pains of this tormenting malady, and its debilitating effects, much more intolerable to one of his active habits and strong animal spirits.

The directions of his will were characteristic of his tastes as a lover of science, and its provisions truly

marked the man, ever careless of the fame of great and good actions, and only intent on performing them.—To Mr. Brown, his librarian, he gave an annuity of £200, with the use of his library and collections, on condition that he should continue his studies in natural history, and assist in superintending the Botanic Garden at Kew.—To Mr. Bauer, who had been his draughtsman for thirty years, he gave an annuity of £300, on condition of his continuing to reside at Kew, and to carry on the drawings of the Kew plants. —He gave the whole collection of the Kew drawings to the King, and strongly recommended the appointment of a resident draughtsman, being of opinion that no botanic garden can be complete without one. He adds, that he had hoped this truth would have obtained from the Government a salary for Mr. Bauer, but if not, he charges it on his Lincolnshire estates. So far the bequests. The directions were, that he should be interred in the parish where he might happen to die; he entreated his relatives to spare themselves the affliction of attending the ceremony; and he earnestly requested that they would not erect any monument to his memory.

He left his widow surviving; she was the daughter and co-heiress of Mr. Hugessen, of Provender in Kent. and had been married to him in 1779. His mother only died in 1804, at a very advanced age; and his sister, who always resided with him, died in 1818. He never had any children; and his large estates devolved upon his wife's relations, the Knatchbull and Stanhope families, the late Sir E. Knatchbull having married Lady Banks' sister, and co-heiress with her; and his property in Derbyshire and Lincolnshire being left by his will to Colonel Stanhope, brother of the present Earl, who was the grandson of his aunt, Margaret Eleanor Banks, by Henry Grenville, brother of Earl Temple. Sir E. Knatchbull, his nephew by marriage, was appointed executor of his last will.

APPENDIX.

I.

Capt. Cook to Mr. Banks.

"Dear Sir,

"I received a note from Mr. Marsh, of the Victualling Office, wherein he desires that we will call upon him on Friday morn, as he is obliged to attend at the Admiralty on Thursday. I left a line at your house yesterday, desiring to know your sentiments concerning a stove for the cabin, it being necessary the officers of Deptford Yard should know how to act. If you approve of a green baize floor-cloth for the great cabin, I will demand as much cloth from the Yard as will make one. As you mean to furnish the cabin well, I think you should have brass locks and hinges to the doors, &c., this, however, will be a private affair of your own, as nothing of this kind is allowed; the round-house will be fitted in this manner at my expense.—Thus far I had got with this letter when your note arrived: I think it a good thought to take Mr. Buzagio's stove with you, as it may be very useful on many occasions. I shall go to Deptford to-morrow to give directions about the other. Whenever it is certain that Dr. Lynd goes with us, I beg you will let me know by the penny post. My respects to the Dr.,* and am,

"Dear Sir,

"Your very humble servant,

"James Cook.

"Monday Evening, Six o'clock."

Dr. Priestley to Mr. Banks.†

"Dear Sir, "Leeds, December 10, 1771.

"After the letter which I received about a fortnight ago from Mr. Eden, who informed me that he wrote at your

* Dr. Solander. † See 'Life of Priestley.'

request, I cannot help saying that yours and his, which I have now received, appear a little extraordinary. In the former letter there was far from being the most distant hint of any objection to me, provided I would consent to accompany you. You now tell me that, as the different Professors of Oxford and Cambridge will have the naming of the person, and they are all clergymen, they may possibly have some scruples on the head of religion; and that, on this account, you do not think you could get me nominated at any rate, much less on the terms which were first mentioned to me. Now what I am, and what they are, with respect to religion, might easily have been known before the thing was proposed to me at all. Besides, I thought that this had been a business of *philosophy* and not of *divinity*. If, however, this be the case, I shall hold the Board of Longitude in extreme contempt, and make no scruple of speaking of them accordingly, taking it for granted that you have just ground for your suspicions.

"I most sincerely wish you a happy voyage, as I doubt not it will be greatly to the emolument of science; but I am surprised that the persons who have the chief influence in this expedition, having (according to your representation) minds so despicably illiberal, should give any countenance to so noble an undertaking; and I am truly sorry that a person of your disposition should be subject to a choice restricted by such narrow considerations.

"I am, dear Sir,
"Your obedient, humble servant,
"J. PRIESTLEY."

"TO GEO. ROSE, ESQ., TREASURY CHAMBERS.

"MY DEAR SIR, "SOHO SQUARE, March 2nd, 1787.

"By an Arrêt, dated April 23, 1775, M. Turgot took off all kinds of droits from books imported under the most general description, as "*reliés ou vieux ou neufs*." I wish I had his Eloge, in which the compliments paid him on the occasion are pretty. I have sent to borrow it from Lord Lansdowne's library, but his Lordship has not yet risen after the fatigues of last night. The exemption is still continued, as may be seen in the *Recueil des Droits*, printed last year.

"Far be it from me to press the subject. I shall always consider literature as under great obligations to Mr. Pitt, who scrupled not a moment to forbid the additional tax intended by the compilers of the rate-book; but sure he might, by giving up a very small receipt, oblige a numerous body whose claim of exemption has been acceded to by the French nation, which circumstance, however, I only wish to bring forward as my apology for the trouble I have given.

"Should it be thought expedient to continue the tax upon bound books, lest the bookbinders might suffer, a clause allowing a quarter of a hundred instead of under ten pounds for each man's private books would make strangers easy; and in that case, if the unbound were quite given up, with only the proviso against books of which editions are extant, printed in England, we should be secured from piracy, and a small portion indeed of revenue sacrificed.

"In France those who attempt to import a pirated edition are very roughly handled by other laws.

"Believe me, dear Sir,
"Most faithfully,
"Your obliged humble servant,
"Jos. Banks."

"Mr. Parkinson, "July 17th, 1809.

"I am not certain that I well understand what Mr. Lacy has been doing in his capacity of Inspector; his aim I conclude in surcharging my tenant at a higher rent than my farms are let for, is a trick by which he expects to obtain an increased tax on the lands I hold in my own hands; he dares not, I am confident, venture even to suppose that I have let my land collusively, or received any fine or other consideration in hand to lower the amount of the reserved rent.

"I let my land, as you know, at a rent which I think and believe to be its real value, that is, I take to myself such a share of the produce as ought in my opinion to belong to the landlord, leaving the tenant what in my opinion he ought to have as his share, and I do not calculate this idly or by guess. You have laid before me on divers occasions what the produce of a farm will be, if well managed in an average season, stating the gross amount of receipt

on each article of produce valued at an average price, such as you and I think likely to be permanent; of this sum you and I allot what we think necessary for the cultivation of the farm, what we think the tenant ought to have to pay his household, pay his tithes, rates and taxes, and allow some savings to him if he is industrious and frugal; the rest is apportioned to me as my share, and more than that portion no landlord ought to take, and in fact most landlords of gentlemen's families and liberal educations are contented with such a proportion.

"Those who exact higher rents, who have no feeling for the oppression of their tenants, who employ attorneys as their stewards, or keep lawyers in pay by retaining fees to watch over their interests, and recover arrears from their tenants when they can no longer support their families, and who are at last compelled to deduct from their net profits the cost of law charges, the losses suffered by tenants unable to pay the whole of the arrears, and the increase of poor's rates on their estates, which must arise from the persons who used to pay them being reduced to the necessity of receiving them or of starving, are surely not to be considered as examples which Government wishes to hold up for imitation, and compel humane men to adopt.

"If I am mistaken in the rate I have set upon my lands as rent, the Commissioners will by enforcing the surcharges put me right; I must in that case raise the farms not in lease to the rent they consider as a proper one: Government will in that case have the credit of raising my estate very much to my emolument, as I must receive 18*s.* for every 2*s.* they get, but the whole of the unpopularity of the measure must rest on their shoulders.

"Thus much for my tenants: for the lands I hold in hand, I have no objection, if the Commissioners choose to rate me so, to pay at the value fixed upon it by the quality men; they acted under parliamentary sanction, and upon their oaths; they are persons over whom I have no kind of influence, and if I had, I should have urged them to value as high as possible, because in that case I should get the greater share of the Fen to be divided. I have, however, entirely acquiesced in their valuation, and have received my share of Wildmore Fen at their rate: am I therefore to receive under

the sanction of one Act of Parliament at a low rate, and to be taxed under another Act at a high one? English policy does not admit such an idea, and I doubt whether it would be well received in Turkey or in Barbary; besides, no increase in the value of stock has taken place since this valuation was made.

"I thank you much for having provided me with a pony. I can do without it till I come to Revesby: you will by that time be perfectly acquainted with its qualities. Perfect surefootedness is my great object. I am weak; and if a horse should fall under me, I cannot hold myself upon him.

"You were right in telling the Fen Commissioners, that if they do not allow to the soke their just rights over the Fodder Fen, I must seek justice elsewhere. The Fodder Dike is so strong an argument, and the constant usage, that it would be in truth a crying injustice to be blind, as they seemed to wish to be, to a right so substantially established.

"If you wish for further instructions on the subject of the surcharges, be good enough to write to me, and state what your opinion is, and what other people think. I could easily fill another sheet of paper, for I am sure that Government never meant a surcharge on property let honestly and fairly, however low that rent might be; all they sought for was to check collusion and other kinds of cheating.

"Your sure friend,
"Jos. Banks."

SIR J. BANKS TO LORD GRENVILLE.

"My Lord, "Soho Square, July 20th, 1796.

"When I had the honour near three weeks ago of waiting upon your Lordship, by your appointment, on the business of M. de Billardiere, I was in hopes I had convinced your Lordship that the measure of returning to that gentleman the collections of natural history he had made during his employment as a naturalist on the voyage of discovery sent from France for the purpose of inquiring into the fate of the late M. de Peyrouse, was a measure likely to do honour to the national character of the English,

as a people loving science and abounding with generosity, as well as with justice, and liable to no reasonable objection whatever.

"I was in hopes also that your Lordship would consider it as creditable to His Majesty's Ministers to grant in this instance a truce to the unfortunate animosities at present subsisting between England and France, by following the precedents of their predecessors in the case of M. de Condamine, of the French nation under their late form of government in that of Captain Cook, and under their present one in the mistaken instance of M. Spillard.

"I hope I have not been mistaken, though your Lordship will allow that I have reason to fear the contrary, because you promised me a speedy answer, and I have not heard from your Lordship since. Respecting the opinion of M. de Billardiere having received any special commission or enjoyed any salary from the late King of France, I have made every inquiry in my power without learning anything to make me believe that to have been the case; the late King did certainly draw up private instructions for M. de Peyrouse, and this has probably been the origin of the mistake.

"Allow me then, my Lord, to request a speedy answer to this interesting subject, and to deprecate a refusal. M. de Billardiere is, as I am informed by printed documents, at this time Director of the Botanic Garden at Paris, at the head of his department of science, and in a country where, however humanity may have been outraged by popular leaders, science is held in immeasurable esteem, he will have it in his power to appeal to Europe, if in his case the justice is refused which was formerly granted by us to De Condamine, and by his countrymen to Cook; and I fear Europe, if such an appeal is made, is more likely to take part with the complainant than with a nation which for the first time refuses a reasonable indulgence to science in alleviation of the necessary horrors attendant on a state of warfare.

"As I possibly may have occasion to correspond with your Lordship on another subject similar in principle to that now under consideration, I take the liberty to state as follows:—

"The French either have, or will soon solicit from His

Majesty's Ministers, a passport for a ship intended to be sent to Trinidad for the purpose of bringing away a collection of living plants deposited there for fear of capture. I hope, my Lord, that this request will be readily granted. The credit Europe has given to the English for having brought useful plants from the South Seas to their colonies in the west, has fully shown that all good men respect the extensive benevolence of increasing the food of mankind, by removing useful plants to countries where Nature has not provided them; and our amiable Monarch has set the example of sending useful plants from his Botanic Gardens to the East, to the West Indies, and to Africa.

"Besides, my Lord, the very application virtually offers, during the horrors of a war unprecedented in the mutual implacability of the parties engaged, an unconditional armistice to science; surely, my Lord, such an offer should not be neglected; the ready acceptance of it may be the signal of the return of the dawnings of good will towards men, and produce consequences, in the present position of Europe, valuable beyond appreciation to all the nations who inhabit it.

"I have the honour to be, my Lord, with due respect and unfeigned esteem,

"Your Lordship's obedient,
humble servant,
"Jos. Banks."

Sir J. Banks to M. Charretie.

"Sir, "August 10th, 1796.

"I have great pleasure in acquainting you that I am now fully empowered to deliver to you the collection made by M. de Billardiere, in order to their being put on board the next Cartel ship, and conveyed by you to that gentleman.

"If you will do me the honour of calling in Soho Square, at any time to-morrow before twelve o'clock, I shall have great pleasure in consulting with you on the proper mode of packing them safely for the voyage, and also on the time which you choose to have them conveyed to the place from

whence they are to be put on board; matters which, I apprehend, cannot be so well settled any where as on the spot where the collection now is."

M. Charretie to Sir J. Banks.

"Walcot Place, le 10 Août, 1796.

"M. Charretié fait bien ses complimens à Monsieur le Chevalier Banks, et ne doutant pas que ce ne soit à ses démarches que le Gouvernement Français soit redevable de la remise de la collection de M. la Billardière, il peut être persuadé de la reconnoissance du Directoire Exécutif. M. Charretié aura l'honneur d'aller demain avant midi témoigner à Monsieur Banks toute sa gratitude particulière pour ses bons offices, et conférer des moyens les plus propres à faire l'envoi de la collection dont il s'agit."

II.

The very imperfect manner in which the attempts to write Sir Joseph Banks's Life have been made we have already had occasion to remark, as well as the errors which have been introduced into the accounts hitherto given of that eminent person. There is but too much reason to fear that this work ill supplies the defect in our scientific history, owing, among other things, to his having strictly ordered all his letters and other manuscripts to be destroyed. But errors have been corrected, and it is hoped that some important particulars have been given.

Among the accounts hitherto offered to the world those of the French writers are beyond all comparison the most erroneous and indeed fanciful. The 'Biographie Universelle' may be cited as peculiarly abounding in such inventions.—The statement that Sir Joseph allowed Dr. Solander a salary or pension of 400*l* a-year I believe to be wholly groundless; the sum would have been preposterous, especially considering that the Doctor enjoyed a considerable place in the British Museum.—The institution of the Copley Medal is said to be for "the experiments the most useful to the preservation of lives," whereas it is for the "best paper on experimental

philosophy in the year."—The group called the "Society Islands" is said to derive its name from the "caractère doux et sociable des habitans," and Otaheite is said to be the chief. Now Otaheite is 150 miles distant, and belongs to the Friendly Islands; and Cook tells us himself that he named the others Society Islands, six in number, "on account of their being contiguous to each other."

I am truly happy to announce my hope that a fuller Life of Sir J. Banks, being in such excellent hands as those of Mr. Dawson Turner, of Great Yarmouth, will be finished by that much and justly respected gentleman.

D'ALEMBERT.

THE pleasures of a purely scientific life have often been described; and they have been celebrated with very heartfelt envy by those whose vocations precluded or interrupted such enjoyments, as well as commended by those whose more fortunate lot gave them the experience of what they praised; but it may be doubted, if such representations can ever apply to any pursuits so justly as to the study of the mathematics. In other branches of science the student is dependent upon many circumstances over which he has little control. He must often rely on the reports of others for his facts; he must frequently commit to their agency much of his inquiries; his research may lead him to depend upon climate, or weather, or the qualities of matter, which he must take as he finds it; where all other things are auspicious, he may be without the means of making experiments, of placing nature in circumstances by which he would extort her secrets; add to all this the necessarily imperfect nature of inductive evidence, which always leaves it doubtful if one generalization of facts shall not be afterwards superseded by another, as exceptions arise to the rule first discovered. But the geometrician* relies entirely on himself; he is absolute master of his materials; his whole investigations are conducted at his own good pleasure, and under his own absolute and undivided

* It may be as well to adopt the expression always used on the continent, to denote the cultivation of mathematical science:—"Ce grand géomètre," is a phrase now universally understood and applied to mathematicians of every description.

control. He seeks the aid of no assistant, requires the use of no apparatus, hardly wants any books; and with the fullest reliance on the perfect instruments of his operations, and on the altogether certain nature of his results, he is quite assured that the truths which he has found out, though they may lay the foundation of further discovery, can never by possibility be disproved, nor his reasonings upon them shaken, by all the progress that the science can make to the very end of time.

The life of the geometrician, then, may well be supposed an uninterrupted calm; and the gratification which he derives from his researches is of a pure and also of a lively kind, whether he contemplates the truths discovered by others, with the demonstrative evidence on which they rest, or carries the science further, and himself adds to the number of the interesting truths before known. He may be often stopped in his researches by the difficulties that beset his path; he may be frustrated in his attempts to discover relations depending on complicated data which he cannot unravel or reconcile; but his study is wholly independent of accident; his reliance is on his own powers; doubt and contestation and uncertainty he never can know; a stranger to all controversy, above all mystery, he possesses his mind in unruffled peace; bound by no authority, regardless of all consequences as of all opposition, he is entire master of his conclusions as of his operations; and feels even perfectly indifferent to the acceptance or rejection of his doctrines, because he confidently looks forward to their universal and immediate admission the moment they are comprehended.

It is to be further borne in mind, that from the labours of the geometrician are derived the most important assistance to the researches of other philosophers, and to the perfection of the most useful arts. This consideration resolves itself into two: one is the plea-

sure of contemplation, and consequently is an addition to the gratification of exactly the same kind, derived immediately from the contemplation of pure mathematical truth; much, indeed, of the mixed mathematics is also purely mathematical investigation, built upon premises derived from induction. The other gratification is of a wholly different description; it is connected merely with the promotion of arts subservient to the ordinary enjoyments of life. This is only a secondary and mixed use of science to the philosopher; the main pleasure bestowed by it is the gratification which, by a law of our nature, we derive from contemplating scientific truth, when indulging in the general views which it gives, marking the unexpected relations of things seemingly unconnected, tracing the resemblance, perhaps identity, of things the most unlike, noting the diversity of those apparently similar. This is the true and primary object of scientific investigation. This it is which gives the pleasure of science to the mind. The secular benefits, so to speak, the practical uses derived from it, are wholly independent of this, and are only an incidental, adventitious, secondary advantage. I have fully explained this doctrine in the Preliminary Discourse to the works of the Society for the Diffusion of Useful Knowledge, and in the Introduction to the 'Political Philosophy.' It never had been stated, as far as I know, before; but it rests on such irrefragable principles, that it has not since been called in question.*

It is an illustration of the happiness derived from mathematical studies, that they possess two qualities in the highest degree, not perhaps unconnected with one

* It gave me great pleasure to find it highly approved by my revered friend, Professor Stewart, who regarded it as indeed of more value and originality than I had myself considered it. The outline of it had been read many years before (1798) in a literary society at Edinburgh, to which Lord Jeffrey, Dr. Brown, Mr. Horner, and others belonged. See Appendix to Life of Robertson, vol. ii.

another. They occupy the attention, entirely abstracting it from all other considerations; and they produce a calm agreeable temper of mind.

Their abstracting and absorbing power is very remarkable, and is known to all geometricians. Every one has found how much more swiftly time passes when spent in such investigations, than in any other occupation either of the senses or even of the mind. Sir Isaac Newton is related to have very frequently forgotten the season of meals, and left his food awaiting for hours his arrival from his study. A story is told of his being entirely shut up and disappearing, as it were eclipsed, and then shining forth grasping the great torch which he carried through the study of the heavens; he had invented the Fluxional Calculus. I know not if there be any foundation for the anecdote; but that he continually remained engaged with his researches through the night is certain, and that he then took no keep of time is undeniable. It does not require the same depth of understanding to experience the effects of such pursuits in producing complete abstraction; every geometrician is aware of them in his own case. The sun goes down unperceived, and the night wanes afterwards till he again rises upon our labours.

They who have experienced an incurable wound in some prodigious mental affliction, have confessed, that nothing but mathematical researches could withdraw their attention from their situation. Instances are well known of a habit of drinking being cured by the like means; an inveterate taste for play has within my own observation been found to give way before the revival of an early love of analytical studies. This is possibly a cause of the other tendency, which has been mentioned, the calming of the mind. We have seen in the life of Simson, how he would fly from the conflicts of metaphysical and theological science, to that of necessary truth, and how in those calm retreats he ever

"found himself refreshed with rest."* Greater tranquillity is possessed by none than by geometricians. Even under severe privations this is observed. The greatest of them all, certainly the greatest after Newton, was an example. Euler lost his sight after a long expectation of this calamity, which he bore with perfectly equal mind; both in the dreadful prospect and the actual bereavement, his temper continued as cheerful as before, and his mind, fertile in resources of every kind, supplied the want of sight by ingenious mechanical devices, and by a memory more powerful even than before.† He furnishes an instance to another purpose. Thoughtless and superficial observers have charged this science with a tendency to render the feelings obtuse. Any pursuit of a very engrossing or absorbing kind may produce this temporary effect; and it has been supposed that men occasionally abstracted from other contemplations, are particularly dull of temper. But no one ever had more warm or kindly feelings than Euler, whose chief delight was in the cheerful society of his grand-children, to his last hour, and whose chief relaxation from

* Page 131.

† My late learned and esteemed friend, Mr. Gough, of Kendal, was another example of studies being pursued under the same severe deprivation—but he had never known the advantages of sight, having lost his eyes when an infant, and never had any distinct recollection of light. He was an accomplished mathematician of the old school, and what is more singular, a most skilful botanist. His prodigious memory resembled Euler's, and the exquisite acuteness of his smell and touch supplied in a great measure the want of sight. He would describe surfaces as covered with undulations which to others appeared smooth and even polished. His ready sagacity in naming any plant submitted to his examination was truly wonderful. I had not only the pleasure of his acquaintance, but I have many particulars respecting his rare endowments, from another eminent mathematician, who unites the learning of the older with that of the modern school, my learned friend and neighbour, Mr. Slee, of Tirrel. A detailed account of Mr. Gough's case, by Mr. Slee and Professor Whewell (a pupil of his), would be most curious and instructive. Euler's memory was such, that he could repeat the Æneid, noting the words that begin and end each page. Mr. Gough also was an excellent classical scholar.

his severer studies was found in teaching these little ones.

It has been alleged, and certainly has been somewhat found by experience to be true, that the habit of contemplating necessary truth and the familiarity with the demonstrative evidence on which it rests, has a tendency to unfit the mind for accurately weighing the inferior kind of proof which we can alone obtain in the other sciences. Once finding that the certainty to which the geometrician is accustomed cannot be attained, he is apt either to reject all testimony, or to become credulous by confounding different degrees of evidence, regarding them all as nearly equal from their immeasurable inferiority to his own species of proof—much as great sovereigns confound together various ranks of common persons, on whom they look down as all belonging to a different species from their own. In this observation there is, no doubt, much of truth; but we must be careful not to extend its scope too far, so as that it should admit of no exceptions. The following life affords one of the most remarkable of these; as far as physical science went, Laplace afforded another; in several other branches he was, perhaps, no exception to the rule.*

The hold which their favourite studies have, and keep over geometricians is not the least remarkable proof of the gratification which they are calculated to afford.—I well know, to take one instance within my own observation, that my learned and esteemed friend, the present Lord Chancellor, a most successful student of the mathematics in his earlier years, reverted to the pursuits in which he had so often found delight, long

* It is said that when the Emperor asked him why he had left out the consideration of a Supreme Intelligence in his speculations, he answered that he conceived he could explain the phenomena without that hypothesis. But when we look to his demonstration of the high improbability of the system having been formed without an intelligent cause, (above four millions of millions to one he proves it in his Calcul de Probabilité,) we cannot lend much faith to this Paris anecdote.

after he had held the highest offices and been engaged in the most dissimilar discussions. As late as 1838, when I was engaged in preparing my Analytical Review of the Principia, I found that, by an accidental coincidence, he was amusing his leisure with the calculus long intermitted; and I am sure that he could have furnished as correct and more elegant analytical demonstrations of the Newtonian theorems than I had the fortune to obtain in composing that work.

I have thought it a useful thing to consider the personal history with the scientific achievements of a very great geometrician, with a view to the illustration of these remarks—and I have chosen D'Alembert in preference to Euler or to Clairaut, the two other illustrious analysts of their age, because we have more ample materials for the study. Whatever of peace and comfort he enjoyed, D'Alembert owed to geometry, and confessed his obligations. Whatever he suffered from vexation of any sort, he could fairly charge upon the temporary interruption of his mathematical pursuits. In both portions of his history, therefore, it is likely to prove instructive, and to enforce the doctrine which I have laid down.

Jean le Rond d'Alembert was born on the 17th of Nov., 1717, being a foundling exposed near the church of St. Jean le Rond in Paris, and thus called by the name of the parish, as is usual in most countries. The commissary of the district, before whom the infant was carried, perceiving its feeble and almost dying condition, instead of sending it to the hospital gave the charge to the wife of a poor but honest glazier in the neighbourhood, living in the Rue Michel-le-Comte, for he was acquainted with the good woman's respectability. In a few days the father, M. Destouches, commissary of artillery, came forward to own the child, and made provision for its support. The general belief is, that the exposition had been concerted with the police. But if so, a very needless risk was unac-

countably incurred by exposing so tender an infant in a winter's night, when the parties might have sent it at once to the place where it was destined to be brought up. It is more likely that the mother, afraid of the discovery, if not of the burthen to be thrown upon her, caused the exposure before the father was apprized of the birth having happened, and that as soon as he knew of what had been done, he hastened to send after the person who had been entrusted with the charge. The mother was an unmarried lady, sister to Cardinal Tencin, Archbishop of Lyons, and she was afterwards well known in the circles of Paris as a person of rare talents and accomplishments. Marmontel, in his Memoirs, calls her Madame de Tencin, she having probably in her old age passed by that name; and he relates some of her sayings, of which one is singular in relation to the life of her celebrated son. "Woe to him," said she, "who depends for his subsistence on his pen! The shoemaker is secure of his wages; the bookmaker is not secure of anything." She was wont also to give the result of her experience of men, by recommending persons who lacked friends to prefer choosing them among women, as they are far more zealous to serve those they wish well to; but then, she added, "You must be their friend, and not their lover." She was the author of a novel, 'Les Mémoires du Comte de Cominges,' of which a good judge, Baron Grimm, says, "Il est en possession de faire pleurer." After giving an account of the plot, he adds. "Il a toujours conservé beaucoup de réputation;" and he adds, "Il est de feu Mme. Tencin, sœur du Cardinal de ce nom; cette femme célèbre de plus d'une manière."* This celebrated person was the centre of a distinguished circle of society remarkable for wit, talents, and accomplishments, and after her death Mme. Geoffrin succeeded to her post.

* Corr. iv., 276.

The young D'Alembert, who probably took his name from his nurse, was sent at the age of twelve to the college of the Quatre Nations, where the professors, at that time of warm controversy, belonged to the Jansenist party; and observing the early appearance of genius in their young pupil, they took pains to imbue him with a taste for polemical subjects. In the first year of his studies in philosophy he had written an able and learned commentary of St. Paul's Epistle to the Romans, and as he showed a general capacity for science, the worthy enemies of the Jesuits, finding to their great delight that all profound learning was not engrossed by that body, cherished a hope that a new Pascal had been given to them for renewing their victories over their learned and subtle adversaries. It was with this view that they made him betimes study the mathematics, in which Pascal had so greatly and so early excelled; but they had to deal with a less docile subject than the Port-Royal had formerly found in young Blaise, for they soon perceived that it was in vain to make him quit his figures and his calculations and take to the divinity of the schools; and all their descriptions of the tendency which such studies had to "dry up the heart"* failed to make him abandon what had taken so strong a hold of his whole mind.

When he left the college he showed the first remarkable instance of that kind and even tender disposition which distinguished him through life, and is another example to rescue the geometrician's pursuits from the reproach of hardening the heart. He found himself solitary in the world, without any kindred that acknowledged him, and he reverted to her whose care had reared and comforted his earlier years; he took

* These good fathers did not quite use the language they had employed to turn away Fenelon from "se laisser ensorceler par les attraits diaboliques de la géometrie." Certainly it is a proof of the evil one's ubiquity that we should find him lurking in this of all places.

refuge in the humble dwelling of his nurse, feeling, as he afterwards used to say, that the small income which alone he possessed, a pension of less than fifty pounds settled upon him by his father, would tend to increase somewhat the comforts of the poor people with whom he should board. In that lowly dwelling, a single confined room of which he occupied as his bed-room and his study, he established himself, living with the family and faring as they fared. Here he remained happy and contented for forty years; that is, until his health compelled him to change his abode, when the age of the good woman would not permit her to accompany him. When her husband died she was ill-treated by her grandchildren, who were stripping her of her little property and reducing her to great distress. "Laissez," said D'Alembert, "Laissez tout emporter par ces indignes.—Je ne vous abandonnerai point." Nor did he; he provided for all her wants, and as long as she lived he visited her twice a week, to satisfy himself by his own observation that nothing was wanting of care and attention to secure her comforts. When he became famous his mother's vanity led her to desire his intimacy, a step which natural affection had not suggested. Discovering to him the secret of his birth, she would have had him come and live with her. But he plainly said he regarded the nurse as his mother, and only saw a step-mother in Mme. Tencin.*

In this obscure retreat he devoted himself to his daily pursuits. Such books of mathematics as he could purchase he bought; others he was obliged to consult at the public libraries. From the very small scale of

* "Que me dites-vous là, madame?" he exclaimed; "Ah! vous n'etes qu'une marâtre! C'est la vitrière qui est ma mère." This touching anecdote is differently related by some, as Grimm in his 'Correspondence.' They report the interview as having taken place in presence of the old nurse; that D'Alembert exclaimed, "Ma mère! Ah! la voilà!—Je ne connais point d'autre." And therewithal fell upon her neck and bathed it in his tears.

his library, and from the degree to which in his education and his subsequent studies he was left to himself, it happened that he was constantly making what seemed to him discoveries, and as constantly finding in some book, which he had not before been able to consult, that he had been anticipated. He drew from hence a very inaccurate inference; he supposed that nature had refused him the gift of original genius, and that he must rest satisfied with studying what others had discovered. But this gave him no pain; the gratification of investigating mathematical truth was all he desired, and with having that enjoyment in his studies he was abundantly contented, regarding the glory of first making the step a very subordinate consideration, and esteeming the pleasure of the contemplation a sufficient reward of his labour. This most interesting circumstance was related by himself to M. Condorcet, a profound and accomplished geometrician, who enjoyed his entire confidence, and succeeded him in the Academy.

While, however, his time thus passed in tranquil enjoyment, the very moderate income which he possessed rendered it advisable that he should seek for some means of increasing it, and rendering himself independent, as well as helping more actively those he cared for. He was advised to study the law, and in the law he took his degrees. But nothing could less suit his taste than this study, and he changed it for that of medicine.

Finding that his passion for the mathematics interfered with this pursuit, he adopted the singular expedient of sending his books to a friend's house, that he might keep temptation out of his way. The resolution was, that he should not be allowed to have them again until he had taken his Doctor's degree. For some time this arrangement succeeded; but his mind hankering after the forbidden scene, he would be ever haunted with the vision of some quantity, some func-

tion whose exact exponents had escaped him, some formula of which he could not recal the solution; he would then get back a volume, and thus one by one the whole of his little stock of precious learning returned into his possession, while the title of Doctor, the quantity, the arbitrary function M.D., remained without any approximation. He then fairly gave up the struggle, and devoted his life to geometrical pursuits.

The account which he always gave of his following years was one glowing with the recollection of the purest happiness; and he was fond of dwelling upon all its details. Perfectly tranquil, without a thought of wealth or power or distinction, his whole enjoyments of an intellectual cast, his existence was as entirely that of a philosopher as ever fell to the lot of any one in ancient or in modern days.—"I awoke," he would say, "every morning to look back, with a feeling of gladness in my heart, on the investigation which I had begun over-night, and exulting in the prospect of continuing it to the result as soon as I rose. When I stopt my operations for a few moments to rest myself, I used to look forward to the evening when I should go to the theatre and enjoy another kind of treat, but also aware that between the acts I should be thinking on the greater treat my next morning's work was to afford me."—It was at this period of his life, at once glorious and happy, though still passed in obscurity, that the good old woman whom he loved as a mother, and who doated on him as a son, would say when any one told her of the great renown he was preparing for his name, "Oh, you will never be any thing better than a philosopher. And what's a philosopher? A foolish body who wears his life out to be spoken of after he's dead."

His studies, however, as might well be expected, soon proved eminently successful. In 1739 he presented to the Academy of Sciences a paper containing

some important corrections of errors into which Père Reynau had fallen in his treatise 'Analyse Démontrée;' these errors D'Alembert had discovered when studying the book in order to learn the calculus, and they related to the integrals of binomials.* This memoir gave a most favourable impression of his capacity to the eminent men who at that time formed the mathematical portion of this illustrious body, Mairan, Cassini, Camus, Fouchy, above all Clairaut, then in the meridian of his great and just renown. The young analyst became their acquaintance first, then their friend. In 1741 he was admitted into the Society, at the early age of twenty-four. Excepting Clairaut, who for the maturity of his extraordinary faculties at an early age is an exception to all rules, no one had ever been an Academician so young. Clairaut had by Royal Ordinance, dispensing with the rule that required the age of twenty complete, been admitted an Adjoint at eighteen, and an Associate at twenty; but at twelve he had presented a memoir upon an important analytical subject, and at the same early age he had made some progress in his greatest work, the 'Courbes à double Courbure,' which was nearly completed at thirteen, and at sixteen was actually published.†

In 1743, two years after D'Alembert entered the Academy, appeared his 'Traité de Dynamique,' which at once placed him in the highest rank of geometricians. The theory is deduced with perfect precision, and with as great clearness and simplicity as the subject allows, from a principle which he first laid down

* The 'Hist. de l'Acad.' 1739, p. 30, records the reading with much approbation of the Memoir of M. le Rond D'Alembert.

† It would certainly have been published in 1725, before he was fourteen years old, but for a violent head-ache which his labours brought on, and which obliged him to give up writing. When his first paper was read at the Academy, the good Father Reynau burst into tears of joy at so marvellous a performance. The 'Hist. de l'Acad.' 1726, p. 45, records his age to have been twelve years eight months.

and explained, though it be deducible from the equality of action and re-action, a physical rather than a mathematical truth, and derived from universal induction, not from abstract reasoning à priori.

The Principle is this, ('Dyn.' pt. 2. ch. i.) If there are several bodies acting on each other, as by being connected through inflexible rods, or by mutual attraction, or in any other way that may be conceived; suppose an external force is impressed upon those bodies, they will move not in the direction of that force as they would were they all unconnected and free, but in another direction; then the force acting on the bodies may be decomposed into two, one acting in the direction which they actually take, or moving the bodies without at all interfering with their mutual action, the other in such direction as that the forces destroy each other, and are wholly extinguished; being such, that if none other had been impressed upon the system, it would have remained at rest.* This principle reduces all the problems of dynamics to statical problems, and is of great fertility, as well as of admirable service in both assisting our investigations and simplifying them. It is, indeed, deducible from the simplest principles, and especially from the equality of action and re-action; but though any one might naturally enough have thus hit upon it, how vast a distance lies between the mere principle and its application to such problems, for example, as to find the locus or velocity of a

* Lagrange's statement of the principle is the most concise, but I question if it is the clearest, of all that have been given. "If there be impressed upon several bodies, motions which they are compelled to change by their mutual actions, we may regard these motions as composed of the motions which the bodies will actually have, and of other motions which are destroyed; from whence it follows, that the bodies, if animated by those motions only, must be in equilibrio." ('Méc. An.' vol. i., p. 289, Ed. 1811.) It is not easy to give a general statement of the principle, and I am by no means wedded to the one given in the text. A learned friend has communicated one which the reader will find in Appendix I., together with a statement, by another excellent geometrician, of the real benefit derived from the Principle.

body sliding or moving freely along a revolving rod, at the extremity of which rod a fixed body moves round in a given plane—a locus which the calculus founded on the Principle shows to be in certain cases the logarithmic spiral.*

No one can doubt that the Principle of D'Alembert was involved in many of the solutions of dynamical problems before given. But then each solution rested on its own grounds, and these varied with the different cases; their demonstrations were not traced to and connected with one fundamental principle. He alone and first established this connexion, and extended the Principle over the whole field of dynamical inquiry.

The 'Traité' contains, further, (part 1. ch. ii.), a new demonstration of the parallelogram of forces. The reason of the author's preference of this over the common demonstration, is not at all satisfactory. His proof consists in supposing the body to move on a plane sliding in two grooves parallel to one side of the parallelogram, and at the same time carried along in the direction of the other side. This is not one whit more strict and rigorous than the ordinary supposition

* The general equation is $d^2 y = \frac{y\, d x^2}{a^2} + \frac{2\, D\, y\, d y^2}{A\, a^2 + D\, y^2}$ in which y is the distance of the moving body D from the fixed point, or the length of the rod, at the end of which is the body A, describing an arch of a circle, and x that arch. The velocity of D is likewise found in terms of the same quantity.

I have freely admitted that the principle of D'Alembert flows from the equality of action and re-action; but nothing can be more incorrect than the remark made by a learned critic, ('Quarterly Review,' vol. v., p. 345,) that "this boasted principle is little more than Newton's third law of motion modified so as to suit the algebraical method of investigating propositions;" on which is grounded a complaint that the French, while praising D'Alembert, never mention Newton, the real author of the principle. The third law of motion was assuredly no discovery of Sir I. Newton; and as certainly the praise of the step made was due to D'Alembert, unless indeed Bernouilli, and still more Fontaine, in some sort anticipated him, probably without his being aware of it. The critic to whom I allude is well founded in urging the like complaints against the French chemists for omitting all mention of Black. But Fourcroy and others are great exceptions. See Life of Lavoisier, p. 319.

of the body moving along a ruler parallel to one side, while the ruler at the same time moves along a line parallel to the other side. Indeed I should rather prefer this demonstration to D'Alembert's.

The 'Traité de Dynamique' appeared in 1743, and in the following year its fundamental principle was applied by the author to the important and difficult subject of the equilibrium, and motion of fluids, the portion of the 'Principia' which its illustrious author had left in its least perfect state. Pressed by the difficulty of the inquiry, which is one of the most important in Hydrodynamics, the motion of a fluid through an orifice in a given vessel, and despairing of the data affording the means of a strict and direct solution, Newton had recourse to assumptions marked by the most refined ingenuity, but admitted to be gratuitous and to be unauthorized by the facts. The celebrated Cataract is of this description. He supposes ('Principia,' lib. ii. prop. 36,) that a body of ice shaped like the vessel, comes in contact with the upper surface of the liquid and melts immediately on touching it, so as to keep the level of the fluid always the same, and that a cataract is thus formed, of which the upper surface is that of the fluid, and the lower that of the orifice. His first investigation assumed the issuing column to be cylindrical, but he afterwards found that the lateral pressure and motion gave it the form of a truncated cone which he called a vein; and his correction of the former result was a matter of much controversy among mathematicians. Daniel Bernouilli at first maintained it to be erroneous against Riccati and others, but he afterwards acquiesced in Newton's view. He, however, always resisted the hypothesis of the cataract, as indeed did most other inquirers. Newton's assumptions, in other parts of this very difficult inquiry, have been deemed liable to the same objections; as where he leaves the purely speculative hypothesis of perfectly uncompressed and distinct particles, and treats of the

interior and minute portions of fluids, as similar to those which we know. (Lib. ii. prop. 37, 38, 39.) It must, however, be admitted as D'Alembert has observed, ('Encyc.' v. 889, and 'Résistance des Fluides,' xvii.) that "those who attacked the Newtonian theory on this subject had no greater success than its illustrious author; some having, after resorting to hypothesis which the experiments refuted, abandoned their doctrines as equally unsatisfactory, and others confessing their systems groundless, and substituting calculations for principles."

Such was the state of the science when D'Alembert happily applied his Dynamical principle to the pressure and motion of fluids, and found that it served excellently for a guide, both in regard to non-elastic and elastic fluids. In fact the particles of these being related to one another by a cohesion which prevents them not from obeying an external impulse, it is manifest that the principle may be applied. Thus, if a fluid contained in a vessel of any shape be conceived divided into layers perpendicular to the direction of its motion, and if v represent generally the velocity of the layers of fluid at any instant, and $d\,v$ the small increment of that velocity, which may be either positive or negative, and will be different for the different layers, $v \pm dv$ will express the velocity of each layer as it takes the place of that immediately below it; then if a velocity $\mp dv$ alone were communicated to each layer, the fluid would remain at rest. ('Traité de Fluides,' Liv. ii. ch. 1. Theor. 2.) Thus the velocity of each part of the layer being taken in the vertical direction is the same, and this velocity being that of the whole layer itself, must be inversely as its horizontal section, in order that its motion may not interfere with that of the other layers, and may not disturb the equilibrium. This, then, is precisely the general dynamical principle already explained applied to the motion of fluids, and it is impossible to deny that the author is thus enabled to demon-

strate directly many propositions which had never before been satisfactorily investigated. It is equally undeniable that much remained after all his efforts incapable of a complete solution, partly owing to the inherent difficulties of the subject from our ignorance of the internal structure and motions of fluids, and partly owing to the imperfect state in which all our progress in analytical science still has left us, the differential equations to which our inquiries lead having, in very many cases, been found to resist all the resources of the integral calculus.

This remark applies with still greater force to his next work. In 1752, he published his Essay on a new theory of the Resistance of Fluids. The great merit of this admirable work is that it makes no assumption, save one to which none can object, because it is involved in every view which can well be taken of the nature of a fluid; namely, that it is a body composed of very minute particles, separate from each other, and capable of free motions in all directions. He applies the general dynamical principle to the consideration of resistance in all its views and relations, and he applies the calculus to the solution of the various problems with infinite skill. It is in this work that he makes the most use of that refinement in the integral calculus of which we shall presently have occasion to speak more at large, as having first been applied by D'Alembert to physical investigation, if it was not his own invention. But the interval between 1744 and 1752 was not passed without other important contributions to physical and analytical science. In 1746, he gave his Memoir on the general theory of Winds, which was crowned by the Royal Academy of Berlin. The foundation of this able and interesting inquiry is the influence of the sun and moon upon the atmosphere, the aërial tides, as it were, which the gravitation towards these bodies produces; for he dismisses all other causes of aërial currents as too little depending upon any definite opera-

tion, or too much depending upon various circumstances that furnish no precise data, to be capable of analytical investigation. The Memoir consists of three parts. In the *first* he calculates the oscillations caused by the two heavenly bodies supposing them at rest, or the earth at rest in respect of them. In the *second*, he investigates their operation on the supposition of their motion. In the *third*, he endeavours to trace the effects produced upon the oscillations by terrestrial objects. The paper is closed with remarks upon the effects of temperature. The whole inquiry is conducted with reference to the general dynamical principle which he had so happily applied to the equilibrium and pressure of fluids, in his first work upon that difficult subject.

The subject of fluids was, perhaps, the one which most occupied D'Alembert's attention, and for the greatest number of years. His 'Opuscula' contain several interesting tracts upon its various departments, especially the first and fifth volumes, which were published in 1761 and 1768 respectively. But above half the eighth volume relates to the same subject, and it appeared as late as 1780, so that this inquiry had retained its hold on his mind for a period of nearly forty years.*

* The readers of D'Alembert's papers on these subjects will have real obligations to Bossut, if they read with D'Alembert that great didactic writer's admirable treatise, 'Hydrodynamique,' second edition. He was an intimate friend and, indeed, may be said to have been a pupil, of D'Alembert and of Condorcet. His 'Calcul Intégral et Différentiel,' is also a truly excellent and useful work. Of the four great elementary treatises on this subject, Lacroix's, Bougainville's, Cousin's, and Bossut's, the last appears to me the best; but I am aware of the high opinion which D'Alembert entertained of Bougainville's. He was accustomed to refer to Bossut those who applied to him for explanations of his writings, as Newton did to Demoivre.—Why, may it be permitted us respectfully to ask, why will so many mathematicians fancy it beneath them to write clearly, simply, and, as didactic matter should be written, intelligibly—and always proceeding from what is known and explained to what is not, without anticipation? Surely Bossut was as great a geometrician as themselves, and he condescended to write as if he were teaching and not commenting, alluding, or referring.

We may further observe, that the extreme interest which he took in it seems to have made him somewhat susceptible, when he conceived others had not done justice to his labours in this favourite department of science. Not only is he anxious, perhaps beyond what is altogether beseeming the calm and disinterested love of investigation, to secure the admission of his claims as the original discoverer; but we sometimes find him even querulous, as to the remarks of others, and complaining of them for not rendering him justice. In the 'Opuscula,' tom. i., p. 158, we have not only an anxious statement of his having been the first to use the method employed in the 'Essai sur la Résistance des Fluides,' and adding, that "great geometricians had so much valued it as to apply it in their inquiries;" but he objects to their having maintained that his theory was capable of greater extension than he had given it, and observes that he had turned it to other inquiries which had escaped them. In the able and learned article Hydrodynamique, in the 'Dict. Encyc.,' vol. viii., p. 373, he attacks Euler for supposing, in his 'Mémoire Acad. de Berlin,' 1755, that D'Alembert's method in his Essai was not general; and he adds, "Il me semble que M. Euler auroit dû rendre plus de justice à mon travail sur ce sujet et convenir de l'utilité qu'il en avoit tirée." Assuredly if ever man was above all suspicion of either usurping upon others or overrating his own discoveries, it was this most illustrious geometrician, whose inherent richness of invention made him even blameably careless of his own claims to originality. No one can have contemplated the different periods of D'Alembert's life without being assured that such feelings of jealousy and irritation as appear in the passages just now cited, were not congenial to his nature and to his earlier habits, when his darling science maintained undisputed possession of his mind, excluded all anxiety save in the search after truth, and calmed every temporary ruffling

of his composure. The dates these passages bear, of 1761 and 1765, long after his admission into the circle of Madame du Deffand, and his participation in the labours and factions of the Encyclopædists, the Diderots, the Holbachs, the Voltaires, show sufficiently that he had exchanged the peace of geometry for the troubled existence of côterie and party.

We ought, while on this subject, to add the just and judicious remark of Bossut on the circumstance of James Bernouilli having anticipated in some sort D'Alembert's method of treating dynamical problems: "That the latter seemed to prove, by the numerous and important applications which he had made of his Principle, that in all probability he owed the discovery of it solely to himself." ('Hydrodyn,' I., xv.)

In treating of Hydrodynamics D'Alembert had found the ordinary calculus insufficient, and was under the necessity of making an important addition to its processes and its powers, already so much extended by the great improvements which Euler had introduced. This was rendered still more necessary when, in 1746, he came to treat of the winds, and in the following year when he handled the very difficult subject of the vibration of cords, hitherto most imperfectly investigated by mathematicians.* In all these inquiries the differential equations which resulted from a geometrical examination of the conditions of any problem, proved

* Taylor ('Methodus Incrementum') had solved the problem of the vibrating cord's movement, but upon three assumptions—that it departs very little from the axis or from a straight line, that all its points come to the axis at the same moment, and that it is of a uniform thickness in its whole length. D'Alembert's solution only requires the last and the first supposition, rejecting the second. The first, indeed, is near the truth, and it is absolutely necessary to render the problem soluble at all. The third has been rejected by both Euler and Daniel Bernouilli, in several cases investigated by them. D'Alembert's solution led to an equation of partial differences of this form $\left(\frac{d^2 y}{d t^2}\right) = a^2 \left(\frac{d^2 y}{d x^2}\right)$ in which t is the time of the vibration, x and y the co-ordinates of the curve formed by the vibration.

to be of so difficult integration that they appeared to set at defiance the utmost resources of the calculus. When a close and rigorous inspection showed no daylight, when experiments of substitution and transformation failed, the only resource which seemed to remain was finding factors which might, by multiplying each side of the equation, complete the differential, and so make it integrable either entirely, or by circular arches, or by logarithms, or by series. D'Alembert, in all probability, drew his new method of treating the subject from the consideration that, in the process of differentiation we successively assume one quantity only to be variable and the rest constant, and we differentiate with reference to that one variable; so that $x\,d\,y + y\,d\,x$ is the differential of $x\,y$, a rectangle, and $x\,y\,d\,z + x\,z\,d\,y + y\,z\,d\,x$ the differential of $x\,y\,z$, a parallelopiped, and so of second differences, $d^2\,z$ being (when $z = x^m$) $= (m^2 - m)\,x^{m-2}\,dx^2 + m\,x^{m-1}\,d^2\,x$. He probably conceived from hence that by reversing the operation and partially integrating, that is, integrating as if one only of the variables were such, and the others were constant, he might succeed in going a certain length, and then discover the residue by supposing an unknown function of the variable which had been assumed constant, to be added, and afterwards ascertaining that function by attending to the other conditions of the question. This method is called that of *partial differences*. Lacroix justly observes that it would be more correct to say *partial differentials;* and a necessary part of it consisted of the *equations of conditions*, which other geometricians unfolded more fully than the inventor of the calculus himself; that is to say, statements of the relation which must subsist between the variables or rather the differentials of these variables, in order that there may be a possibility of finding the integral by the method of partial differences. It appears that Fontaine,* a geometrician of the greatest genius, gave

* Euler had so high an opinion of Fontaine, that in 1751 he told La-

the earliest intimation on this important subject; for the function of one or both variables which is multiplied by $d\,x$ being called M, and that function of one or both which is multiplied by $d\,y$ being called N, the canon or criterion of integrability is that

$$\frac{d\,M}{d\,y} = \frac{d\,N}{d\,x}$$

and we certainly find this clearly given in a paper of Fontaine's read before the Academy, 19th Nov., 1738. It is the third theorem of that paper. Clairaut laid down the same rule in a Memoir which he presented in 1739; but he admits in that Memoir his having seen Fontaine's paper. He expounds the subject more largely in his far fuller and far abler paper of 1740; and there he says that Fontaine showed his theorem to

lande, "If any unexpected discovery shall be made, I believe it will be Fontaine that will make it." (Montucla, iv., 77, note by Lalande.) His name is not even mentioned in the scientific Encyclopædias; nor does Professor Leslie, in his Dissertation to the 'Encyc. Brit.,' show that he had ever heard of it. The delay of the Academy in publishing his papers is apparently suspected by Montucla as having resulted from some unfair feeling towards him. He was a person of the most philosophic habits, living always in the country, where he cultivated a small estate; and having had the misfortune to be involved in an oppressive litigation he appears to have abandoned scientific pursuits during the latter years of his life. (Mem., 1771.) We find him mentioned in some of the contemporary Memoirs, among the very first geometricians. Grimm always treats him as such, and he gives some anecdotes of him. "Fontaine vit à la campagne, et ne vient à Paris que rarement. Il passe auprès des connaisseurs pour le premier géomètre du royaume. Il met du génie dans ses ouvrages, et quand on le connait on n'est pas difficile à persuader sur ce point. C'est un homme d'un tour d'esprit très-piquant. Il réunit une finesse extrême à je ne sais quoi de niais." (Corr. ii., 287.) It must, however, be confessed, that Grimm writes on a subject he knew nothing of, having mixed error with truth. Thus he says of D'Alembert, "Sans avoir rien inventé, il passe pour mettre beaucoup d'élégance et de clarté dans ses ouvrages géométriques," p. 215; thus praising him for exactly that in which he is most deficient, and denying him the originality which was his great merit. Of Clairaut he elsewhere says: "Un très-grand géomètre, presque sur la ligne des Euler, des Fontaine, des Bernouilli, et des D'Alembert. Il avait moins de génie que Fontaine, plus de justesse et de sûreté et moins de pénétration que D'Alembert. Ce dernier a perdu à son mort un rival qui le tenait sans cesse en haleine, et c'est une grande perte." (Corr. iv., 456.) This latter passage is very just in all respects; but the word "presque" at the beginning is altogether absurd.

the Academy the day this second paper of Clairaut's was read—erroneously, for Fontaine had shown it in November, 1738; and had said that it was then new at Paris, and was sent from thence to Euler and Bernouilli. The probability is, that Clairaut had discovered it independent of Fontaine, as Euler certainly had done; and both of them handled it much more successfully than Fontaine. D'Alembert, in his demonstrations, 1769, of the theorems on the integral calculus, given by him without any demonstration in the volume for 1767, and in the scholium to the twenty-first theorem, affirms distinctly that he had communicated to Clairaut a portion of the demonstration, forming a corollary to the proposition, and from which he says that Clairaut derived his equation of condition to differentials involving three variables. It is possible; but as this never was mentioned in Clairaut's lifetime, although there existed a sharp controversy between these two great men on other matters, and especially as the equation of conditions respecting two variables might very easily have led to the train of reasoning by which this extension of the criterion was found out, the probability is, that Clairaut's discovery was in all respects his own.

The extreme importance of this criterion to the method of partial differences, only invented, or at least applied, some years later, is obvious. Take a simple case in a differential equation of the first order,—

$$d\,z = (2\,a\,x\,y - y^3)\,d\,x + (a\,x^2 - 3\,x\,y^2)\,d\,y$$

where $M = 2\,a\,x\,y - y^3$, $N = a\,x^2 - 3\,x\,y^2$

For the criterion $\dfrac{d\,M}{d\,y} = 2\,a\,x - 3\,y^2$

$$\frac{d\,N}{d\,x} = 2\,a\,x - 3\,y^2$$

gives us $\dfrac{d\,M}{d\,y} = \dfrac{d\,N}{d\,x}$,

which shows that the equation $M\,d\,x + N\,d\,y$ is a complete differential, and may be integrated. Thus integrate $(a\,x^2 - 3\,x\,y^2)\,d\,y$, as if x were constant, and add X (a function of x, or a constant), as necessary to complete the integral, and we have

$$a\,x^2\,y - x\,y^3 + X = Z;$$

now differentiate, supposing y constant, and we have

$$\frac{d\,z}{d\,x} = (2\,a\,x\,y - y^3) + \frac{d\,X}{d\,x}$$

(because of the criterion) $= 2\,a\,x\,y - y^3$,

consequently $\frac{d\,X}{d\,x} = o$, and $X = C$, a constant.

Accordingly, $z = a\,x^2\,y - x\,y^3 + C$;

and so it is, for differentiating in the ordinary way, x and y being both variable, we have

$$\begin{aligned} d\,z &= 2\,a\,x\,y\,d\,x + a\,x^2\,d\,y - 3\,x\,y^2\,d\,y - y^3\,d\,x \\ &= (2\,a\,x\,y - y^3)\,d\,x + (a\,x^2 - 3\,x\,y^2)\,d\,y \end{aligned}$$

which was the equation given to be integrated.

To take another instance in which $\frac{d\,X}{d\,x}$, the differential coefficient of the quantity added is not $= o$ or X constant. Let

$$d\,z = y^2\,d\,x + 3\,x^2\,d\,x + 2\,x\,y\,d\,y$$

in which, by inspection, the solution is easy—

$$z = x\,y^2 + x^3 + C$$

Here $\quad M = y^2 + 3\,x^2 \qquad N = 2\,x\,y$

and $\quad \frac{d\,M}{d\,y} = 2\,y = \frac{d\,N}{d\,x}$

So $z = x y^2 + X$, and differentiating with respect to x

$$\frac{d z}{d x} = y^2 + \frac{d X}{d x} = y^2 + 3 x^2$$

Hence $$X = x^3 + C$$

and $$z = x y^2 + x^3 + C,$$

the integral of the equation proposed.

It must, however, be observed of the criterion, that an equation may be integrable which does not answer the condition

$$\frac{d M}{d y} = \frac{d N}{d x}.$$

It may be possible to separate the variables and obtain $X d x = Y d y$, as by transformation; or to find a factor, which, multiplying the equation, shall render it integrable, by bringing it within that condition. The latter process is the most hopeful; and it is generally affirmed that such a factor, F, may always be found for every equation of the first order involving only two variables. However, this is only true in theory: we cannot resolve the general equation by any such means; for that gives us

$$F. \left(\frac{d M}{d y} - \frac{d N}{d x}\right) = N. \frac{d F}{d x} - M \frac{d F}{d y}$$

an expression as impossible to disentangle, it may safely be asserted, as any for the resolution of which its aid might be wanted. It is only in a few instances of the values of these functions (M and N) that we can succeed in finding F. It is quite unaccountable* that Clairaut should, in reference to his equation, which is substantially the same with the above, describe it as "d'une grande utilitè, pour trouver μ" (that is F).

* Mem. de l'Acad. 1740, p. 299.—I find my surprise shared by a very learned mathematician to whom I had mentioned it.

It is here to be observed, that not only Fontaine had, apparently, first of all the geometricians, given the criterion of integrability, but he had also given the notation which was afterwards adopted for the calculus of Partial Differences. ϕ being a function of two variables, x and y, he makes $\frac{d\,\phi}{d\,x}$ stand for the differential coefficient of ϕ when x only varies, and $\frac{d\,\phi}{d\,y}$ for the same differential coefficient when y only varies. Hence he takes $\frac{d\,\phi}{d\,x} \times d\,x$, not, as in the ordinary notation it would be, $= d\,\phi$, the complete differential of ϕ; whereas that differential would, in this solution, be

$$\frac{d\,\phi}{d\,x} \times d\,x + \frac{d\,\phi}{d\,y} \times d\,y\,.$$

Thus if $\phi = x\,y^2$, its complete dif. $d\,\phi = 2\,y\,x\,d\,y + y^2\,d\,x$, but

$$\frac{d\,\phi}{d\,x} = y^2\,.$$

It is quite clear, therefore, that Fontaine gave the notation of this calculus.

But D'Alembert had been anticipated in the method itself, as well as in the notation or algorithm; for Euler, in a paper entitled 'Investigatio functionum ex datâ differentialium conditione,' dated 1734,* integrated an equation of partial differences; and he had afterwards forgotten his own new calculus, so entirely as to believe that it was first applied by D'Alembert in 1744. So great were the intellectual riches of the first of analysts, that he could thus afford to throw away the invention of a new and most powerful calculus! A germ of the same method is plainly to be traced in

* 'Petersburgh Memoirs,' Vol. VII.

Nicolas Bernouilli's paper* in the 'Acta Eruditorum' for 1720, on Orthogonal Trajectories.†

While mentioning Fontaine's great and original genius for analytical investigations, we must not overlook his having apparently come very near the Calculus of Variations. In a paper read at the Academy, 17th February, 1734, we find a passage that certainly looks towards that calculus, and shows that he used a new algorithm as requisite for conducting his operation:— "J'ai été obligé," he says, "de faire varier les mêmes lignes en deux manières differentes. Il a fallu designer leurs variations differemment." "J'ai marqué les unes commes les géomètres Anglais par des fluxions (points); les autres par des differences ($d\,x$) à nôtre manière; de sorte qu'ici $d\,x$ ne sera pas la même chose que $\dot{x}$, $d\,\dot{x}$ que $\dot{x}$," (p. 18.) "Il peut y avoir," he afterwards adds, "des problèmes qui dependroient de cette méthode fluxio-differentielle."

* See, too, the paper in John Bernouilli's Works, Vol. II., p. 442, where he investigates the transformation of the differential equation $d\,x = P\,d\,y$ (P being a function of a, x, and y) into one, in which a also is variable.

† While upon the subject of Partial Differences, we must naturally feel some disappointment that this important subject has not been treated more systematically, especially by later analysts. Some of these, indeed, seem to have formed an extremely vague notion of its nature. Thus Professor Leslie, in his declamatory and inaccurate Dissertation on the progress of mathematical and physical science, ('Encyc. Brit.,' I., 600,) gives a definition of this calculus, which is really that of the fluxional or differential calculus in general, and which, though authorized by an inaccurate passage in Bossut's excellent work, ('Cal. Dif. et Int.,' II., 351,) could never have been adopted by any one who did more than copy after another. He afterwards (p. 606) supposes Clairaut's addition to the inverse square of the distance $\left(\frac{1}{d^2}+\frac{1}{d^4}\right)$ to have been adding what he calls "a small portion of the inverse cube joined to the ordinary term of the inverse square;" and he considers, most unaccountably, that this is not a function of the distance at all. His account of the calculus of variations is equally vague; and the example unhappily chosen is one in which the relations of the co-ordinates do not change, but only the amount of the parameter (Ib., p. 600.) I must also most respectfully enter my protest here, once more, against mathematicians writing metaphorically and poetically, as this learned Professor does in almost every sentence.

Nothing that has now been said can, in any manner, detract from the renown justly acquired by D'Alembert and Lagrange as the first who fully expounded the two great additions to the Differential Calculus, first applied them systematically to the investigation of physical as well as mathematical questions, and therefore may truly be said to have first taught the use of them as instruments of research to geometricians.*

In the year 1746 the Academy of France proposed, as the subject of its annual prize essay for 1748, the disturbances produced by Jupiter and Saturn mutually on each other's orbits. Euler's Memoir gained the prize; and it contains the solution of the famous Problem of the Three Bodies—namely, to find the path which one of those bodies describes round another when all three attract each other with forces varying inversely as the squares of their distances, their velocities and masses being given, and their directions in the tangents of their orbits.† This, which applies to the case of the Moon, would be resolved were we in possession of the solution for the case of Jupiter and Saturn, which, instead of revolving round each other, revolve round the third body. Euler's investigation did not appear quite satisfactory; and, in 1750 the same subject was announced for 1752, when he again carried off the prize by a paper exhausting the subject, and affording such an approximation to the solution as the utmost resources of the integral calculus can give. But while we admit, because its illustrious author himself admitted, the justice of the Academy's views respecting his first solution, we must never forget the

* There was nothing in the observation of Fontaine that can be termed an anticipation of Lagrange, though D'Alembert, unknown to himself, had certainly been anticipated by Euler.

† The problem of the Three Bodies, properly speaking, is more general; but, in common parlance, it is confined to the particular case of gravitation, and indeed of the sun, earth, and moon, as three bodies attracting each other by the law of gravitation, and one of which is incomparably larger than the other two.

extraordinary genius displayed in it. He did not communicate the whole, or even the more essential portion of his investigation, but he afterwards gave it in a paper to the Berlin Academy in 1747, and in another to the Petersburgh Academy in 1750, the first of these containing our earliest view of the variation of arbitrary constants in differential equations, and the development of the radical which expresses the relative disturbance between two planets in a series of sines and co-sines of angles multiples of the elongation, a series so artistly framed that every three consecutive terms are related together in such a manner as to give the whole series from a determination of the first two terms. Clairaut appears to have turned his attention to the same problem some time before Euler. In 1743, he gave a Memoir on the Moon's Orbit, according to the Newtonian theory of gravitation, and it appears in the volume for that year; but this paper must be admitted to have been a somewhat slight performance for so consummate a geometrician. It rather evaded the difficulties of the problem than surmounted by encountering them; for he assumed the orbit of the moon to differ imperceptibly from a circle; and his differential equation could not have been integrated without this supposition. Now, the only assumptions which had been conceived permissible were the incomparably greater mass of one body than those of the two others,* the nearly equal distance of that body from each of the two others, and the almost elliptical path of the one whose orbit was sought, leaving its deviation from that path alone to be sought after. Accordingly, the paper of 1743 did not satisfy its illustrious author, who, in 1747, produced another worthy of the subject and of himself. This was read 15th November,

* In truth, the mass of the sun being 355,000 times that of the earth, and that of the earth being between sixty-eight and sixty-nine times that of the moon, the mass of the sun is twenty-five millions of times greater than that of the moon.

1747, but part of it had been read in August. He asserts positively in a note ('Mém.,' 1745, p. 335,) that though Euler's first paper had been sent in the same year, he had never seen it till after his solution was obtained; therefore, Lalande had no right to state in his note to the very bad edition of Montucla which he published, that Fontaine always said that Clairaut was enabled to obtain his solution by the paper of Euler, (Vol. iv. p. 66.)

At the time that Clairaut was engaged in this investigation, D'Alembert, unknown to him, was working upon the same subject. Their papers were presented on the same day, and Clairaut's solution was unknown to D'Alembert; but so neither could D'Alembert's solution have been known to Clairaut, because the paper is general on the problem, and the section applicable to the moon's orbit was added after the rest was first read, and was never read at all to the Academy. Nothing, therefore, can be more clear than that neither of these great geometricians borrowed from the other, or from Euler. It is just possible that Euler in his complete solution of 1752 might have had the advantage of their previous ones; but as it clearly flowed from his earlier paper, there is no doubt also of his entire originality. Nevertheless, when D'Alembert's name became mixed up with the party proceedings among the literary and fashionable circles of Paris, there were not wanting those who insisted that the whole fame of this great inquiry belonged to Clairaut; and it is painful to reflect on the needless uneasiness which such insinuations gave to D'Alembert. We shall recur to the subject afterwards, and now must continue the history of this problem.

Thus, in investigating this famous "Problem of the Three Bodies," all the three geometricians, without communicating together, took the same general course in the field, like three navigators of consummate skill and most practised experience tracing the pathless

ocean, unseen by one another, and each trusting to his seamanship, his astronomical observations, and his time-keeper, and all of them steering separately the same course. They were each led to three equations, which nearly resembled those obtained by the other two. Of the three equations the most important is—

$$\frac{d^2 u}{d v^2} + u + \frac{T \frac{d u}{d v} - P u}{u^3 \left(h^2 + 2 \int \frac{T}{u^3} d v\right)} = o$$

in which u is the reciprocal of the projection on the plane of the ecliptic of the moon's distance from the earth, v the moon's longitude with respect to the centre of gravity of the earth and moon, P and T the resultants respectively of all the forces acting on the moon parallel and perpendicular to $\frac{1}{u}$, and parallel to the plane of the ecliptic, h an arbitrary constant. P and T being complicated functions of the longitudes of the sun and moon, as well as of the eccentricities of their orbits, have to be developed for the furthet solution of the problem.

Now, it is a truly remarkable circumstance that the conclusion at which all these great men separately arrived was afterwards found to be erroneous. They made the revolving motion of the moon's apogee (or the revolution which the most distant part of her orbit makes in a certain time) half as much as the observations show it to be; and in a revolution of the moon, 1° 30′ 43″, instead of 3° 2′ 32″ the observations giving about nine years for the period, which the revolution really takes, instead of eighteen. Clairaut first stated this apparent failure of the Newtonian theory, and as he had taken pains to make the investigation "avec toute l'exactitude qu'elle demandoit," ('Mém.' 1745, p. 336,) he was with great reluctance driven to conclude

that the doctrine of gravitation failed to account for the progression of the apogee or revolution of the lunar orbit; and if so, as Euler justly observed, (Prix., tom. vii., 'Recherches sur Jupiter et Saturne,' p. 4,) we must have been entitled to call in question the operation of the same principle on all the other parts of the planetary system. Clairaut even went so far as to propose, in consequence of the supposed error, a modification of the law of gravitation; and that we should, instead of considering it as in the proportion of $\frac{1}{d^2}$, (d being the distance,) regard it as proportional partly to $\frac{1}{d^2}$, the inverse square, and partly to $\frac{1}{d^4}$, the inverse fourth power of the distance. But this suggestion was far from giving satisfaction even to those who admitted the failure of the theory. A controversy arose between this great geometrician and a very unworthy antagonist, Buffon, who on vague, metaphysical, and even declamatory grounds, persisted in showing his ignorance of analysis, and his obstinate vanity; nor, though he was by accident, quite right, could any one give him the least credit for his good fortune. Clairaut answered him, and afterwards rejoined to his reply, with a courtesy which betokened entire civility and even respect for the person, with an infinitely low estimation of either his weight or his strength—quantities truly evanescent. At length it occurred to him that the process should be repeated, a course which he certainly must have taken at first had he not naturally enough been misled by the singular coincidence of both Euler and D'Alembert* having arrived at the same conclusion with himself. He found that he ought to have repeated his investigation of the differential

* Euler had stated it incidentally, as regarded the lunar apogee, in his prize memoir, in 1746, on Jupiter and Saturn, but he mentioned it more fully in a letter to Clairaut. ('Mém.' 1745, p. 358, note.)

equation to the radius, after obtaining, by a first investigation, the value of the third term above given in that equation—

$$\frac{T\frac{d\,u}{d\,v} - \&c.}{u^3\,(h^2 + \&c.} \text{ (as above given.)}$$

This omission he now supplied, and he found that the result, when applied to the case, made the progression of the moon's apogee twice as quick as the former operation had given it, or nine years, agreeing with the actual observation. He deposited, in July, 1746, with the secretary of the Academy, as well as with Sir Martin Folkes, president of the Royal Society, a sealed paper containing the heads of his analysis, but delayed the publication of it until he should complete the whole to his satisfaction: a most praiseworthy caution, after the error that had been committed in the first instance. He announced, however, the result, and its confirming the Newtonian theory, in May of the same year; and added, that his reasoning was purely geometrical, and had no reference to vague topics, giving, at the same time, a conclusive exposition of Buffon's ignorance in his hot attack, which showed him to be wholly incapable of appreciating any part of the argument. In May, 1752, the Memoir itself was given to the Academy, and it appears in the volume for 1748.* It is entitled, "De l'Orbite de la Lune, en ne négligeant pas les quarrés des quantités de même ordre avec les forces perturbatrices; which has misled many in their conception of the cause to which the error must be ascribed. But in the volume for 1748, p. 433, he leaves no doubt on that cause; for he states

* For an account of the irregular and irrational manner in which the Memoirs of the Academy were published, see 'Life of Lavoisier.' The inconvenience of it meets us everywhere.

that having originally taken the radius vector r, (the reciprocal of u in our former equation,) $= \frac{k}{1 - \cos. m\, v}$, he now takes fully that reciprocal u or $\frac{k}{r} = 1 - e \cos. m\, v + \beta \cos. \frac{2\, v}{n} - \gamma \cos. \left(\frac{2}{n} - m\right) v + \delta \cos. \left(\frac{2}{n} + m\right) v - \zeta \cos. \left(\frac{2}{n} - 2\, m\right) v$, terms obtained by the first or trial integration, which he had fully explained in his first Memoir to be the more correct mode of proceeding, ('Mém.,' 1745, p. 352;) and the consequence of this is to give the multiplier, on which depends the progression of the apogee, a different value from what it was found to have in the former process. It is never to be forgotten that the original investigation was accurate as far as it went; but by further extending the approximation a more correct value of m was obtained, in consequence of which the expression for the motion of the apogee became double that which had been calculated before.

It should be observed, in closing the subject of the Problem of Three Bodies, that Euler no sooner heard of Clairaut's final discovery, than he confirmed it by his own investigation of the subject, as did D'Alembert. But in the meantime Matthew Stewart, (Life of Simson, p. 137,) had undertaken to assail this question by the mere help of the ancient geometry, and had marvellously succeeded in reconciling the Newtonian theory with observation. Father Walmisley, a young English priest of the Benedictine order, also gave an analytical solution of the difficulty in 1749.

The other great problem, the investigation of which occupied D'Alembert, was the Precession of the equinoxes and the Nutation of the earth's axis, according to the theory of gravitation. Sir Isaac Newton, in the xxxix. prop. of the third book, had given an indirect

solution of the problem concerning the Precession; the Nutation had only been by his unrivalled sagacity conjectured à priori, and was proved by the observations of Bradley. The solution of the Precession had not proved satisfactory; and objections were taken to the hypotheses on which it rested, that the accumulation of matter at the equator might be regarded as a belt of moons, that its movement might be reckoned in the proportion of its mass to that of the earth, and that the proportion of the terrestrial axes is that of 229 to 230; that the earth is homogeneous, and that the action of the sun and moon *ad mare movendum*, are as one to four and a half nearly, and in the same ratio *ad equinoctia movenda*. Certainly the three last suppositions have since Newton's time been displaced by more accurate observations; the axes being found, to be as 298 to 299, the earth not homogeneous, and the actions of the sun and moon on the tides more nearly as one to three. But it has often been observed, and truly observed, that when D'Alembert came to discuss the subject, it would have been more becoming in him to assign his reasons for denying the other hypothesis on which the Newtonian investigation rests, than simply to have pronounced it groundless. However, it is certain that he first gave a direct and satisfactory solution of this great problem; and that he investigated the Nutation with perfect success, showing it to be such that if it subsisted alone, (*i. e.*, if there were no precessional motion) the pole of the equinoctial would describe among the stars a minute ellipse, having its longer axis about 18″ and its shorter about 13″, the longer being directed towards the pole of the ecliptic, and the shorter of course at right angles to it. He also discovered in his investigations that the Precession is itself subject to a variation, being in a revolution of the nodes, sometimes accelerated, sometimes retarded, according to a law which he discovered, giving the equation of correction. It was in 1749 that

he gave this admirable investigation; and in 1755 he followed it up with another first attempted by him, namely, the variation which might occur to the former results, if the earth, instead of being a sphere oblate at the poles, were an elliptic spheroid, whose axes were different. He added an investigation of the Precession on the supposition of the form being any other curve approaching the circle. This is an investigation of as great difficulty perhaps as ever engaged the attention of analysts. It remains to add that Euler, in 1750, entered on the same inquiries concerning Precession and Nutation; and with his wonted candour, he declared that he had read D'Alembert's memoir before he began the investigation.

The only other works of D'Alembert which it is necessary to mention, are his three papers on the integral calculus. Of these one, in the Berlin Memoirs, is replete with improvements extremely important in the methods of integration, and contains a method of treating linear equations of any order that serve as a foundation for the approximate solutions, which are absolutely indispensable to physical astronomy in the present imperfect state of the calculus. The other two are in the French Academy's Memoirs for 1757 and 1769, the latter giving the demonstrations of the theorems on integration contained in the former. It is in the twenty-first of these that he claims having suggested, as we have already seen, to Clairaut his equation of conditions in the case of three variables. The 'Opuscules' contain likewise, especially the 4th, 5th, and 7th volumes, some most important papers on the calculus. Nor must we omit to record that there is every reason to give him credit for having discovered Taylor's Theorem. It is certain that he first gave this celebrated formula complete, having, in the article 'Series' of the 'Encyclopédie,' first given the remaining terms left out by Taylor, and also a demonstration of the whole, better than the original inventor's. Con-

dorcet, who only knew the Theorem from this exposition of it, treats him as certainly being its author; and D'Alembert himself, citing no other discoverer, plainly gives it as altogether his own.*

I have thought it better to pursue the same method in treating of D'Alembert's works that I adopted respecting Voltaire's,† giving all his scientific researches, his important physical and analytical discoveries, in a connected order, and thus avoiding the interruption of the series which an exclusive regard to the chronological succession of his different works on all subjects would have occasioned. We must now return to the history of his life, and the other pursuits with which his severer studies were interrupted, and his enjoyments, as it were, variegated.

In those scientific pursuits, the history of which we have been surveying, he passed the first eighteen years after he left the College, and he passed them in uninterrupted tranquillity and happiness, in tasting the pleasure of contemplating the relations of necessary truths, in adding to the number which had been before ascertained, and in enlarging the sphere of his own usefulness as well as his fame. His existence had been one which the children of this world, the pampered sons of wealth and fashion, the votaries of vulgar pleasure, and the slaves of ordinary ambition would regard as obscure and even wretched; for he had neither wealth nor rank, and all his gratifications were of a purely intellectual kind. But his enjoyment had been unbroken; he had no wants unsupplied; he tasted perfect tranquillity of mind; and his friends, who esteemed him,

* If very small things might be compared to great, I should note the circumstance—the accident, I may well term it—of my having hit upon the Binomial Theorem, and given it as an exercise to Professor Playfair, when attending his class in 1794. He kept my paper, and used to mention this circumstance. He said he concluded I had found it only by induction, which was true. The demonstration is, indeed, of considerable difficulty.

† In Vol. II. of this Series.

were great men of congenial habits. He had now passed his thirty-fifth year—

"Il mezzo di camin di nostra vita."—*Dante.*

His devotion to the mathematics had all along estranged him from those branches of physical science which do not lend themselves to analytical investigation. Indeed, as I have shown in the Life of Simson, he appears even to have disregarded all geometrical inquiries which were unconnected with modern analysis. But he had always cultivated a taste for the belles-lettres, and both read and understood poetry. He was also well acquainted with moral and metaphysical subjects. The singularity is, therefore, great, that he should have had no taste for the inductive sciences. Herein he differed widely from other great geometricians. To say nothing of the greatest of mathematicians, Newton himself, alike of inexhaustible resources in experimental as in analytical and geometrical investigation, Euler and Laplace both were much attached to experimental philosophy. D'Alembert had, moreover, lived in the society of several persons whose pursuits were not at all confined to the mathematics, and with some for whom that science had no attractions. Of these Diderot was his most intimate and earliest friend; and he it was who prevailed upon him to join in the conduct of a great literary undertaking, the first French Encyclopædia. This work was published at Paris from 1751 to 1758; and of these seven volumes D'Alembert and Diderot were the joint editors. D'Alembert also contributed many of the best articles, and wrote the celebrated Preliminary Discourse upon the distribution and the progress of the sciences. The merit of those articles is generally, as might have been expected from such a writer, great in proportion as he exerted himself to elaborate and to finish them. But the best are, as might also have been expected, the mathematical.

The Preliminary Discourse has, in my very humble

opinion, and speaking with an unfeigned respect for both its illustrious author and its eminent eulogists, been praised much beyond its merits. The very ground of those panegyrics, that it traces the invention of the sciences and the arts to the necessities and the desires of individual nature, seems to be a satisfactory proof how fanciful and indeed how confined the whole plan of the work is. Professor Stewart has most justly remarked ('Dissertation, Encyc. Brit. Introd.') that there is in the Discourse a total confusion of two things, in themselves wholly different and which ought to have been carefully kept distinct—the character and circumstances and progress of the individual, and those of the species. It is the scientific advance of the race that the author professes to treat; but he is constantly dealing with the unfolding of the faculties in the man. There arises from hence a most shadowy, indistinct, and vague view of most points discussed. And not unconnected with this confusion is the other main error of the whole treatise, the error into which Bacon had fallen before; the sciences are classified under the heads of memory, imagination, and reason, only Bacon's arrangement revived. But nothing can be more fanciful, nothing less accurate, than such a distribution, which sacrifices sense to point, and sound principles of classification to outward symmetry and affected simplicity. The total want of precision, and of logical arrangement in the details of this division, is indeed striking. Thus under History we have Natural History, or a record of all facts, whether relating to animals, or vegetables, or minerals, or the heavenly bodies, or the elements, as to heat, air, water, meteors. Then in what does this differ from inductive or experimental philosophy, which yet forms a branch of the second great division? Moreover, why are moral facts omitted in the division of History? Then the application of natural powers to different uses is another branch of History, and thus all the arts are introduced under this head. In the

division of Natural Philosophy we find equal want of precision. Can anything be more inexplicable than to find a person, who like D'Alembert was both a mathematician and a metaphysician, treating mathematics as a branch of natural science, as if number, or indeed quantity, could be regarded as a physical existence? Not more happy is the execution of this plan in the moral and intellectual division. These are ranged under the science of Man. Then what place has the subject of instinct, which is just as intellectual a branch as that of reason? Logic is defined to be the science of intellect, or the means of finding truth; Morals, that of the will, or the grounds of virtue. But the Fancy is as much a subject of intellectual science as the Reason. Moreover the moral qualities belong to the understanding. Under Logic he brings hieroglyphics and heraldry ('La Science du Blason'), and also rhetoric, including the art of versification; but poetry belongs to the third great division, Imagination, though oratory is ranged under the second, with Logic.

Thus of this celebrated classification and the famous genealogical tree applied to it, the object of so much self-gratulation with the Encyclopædists, we may fairly judge by its fruits, and they are of but mean value. It shares the same blame, however, with the division of Bacon, the root and seed from which it springs. We find that great master of logic classifying the mechanical arts and history together; nay, in his threefold division of the sciences, according as the Deity, man, or external nature are their objects, he classes intellectual and moral philosophy with anatomy and medicine, optics and acoustics with ethics, the chemical qualities of human bones and blood with human philosophy, that of animal bones and blood with natural philosphy. So D'Alembert not lagging behind his master in paradox, affirms that imagination has the greatest share in metaphysics and geometry of all the sciences connected with reason.

That the celebrated Discourse contains many bold general views, often more bold indeed than considerate, that it abounds with learning, that it is full of ingenious suggestions, is perfectly true. That it is written in a plain, perspicuous style, well suited to a didactic work, is also certain. But that the impression which it produced was owing much more to its large scope, to the amplitude of its range, than to the soundness of its doctrines, or even to any felicity with which these were illustrated, is, I believe, now the opinion of all who impartially consider the subject.

No sooner did the great work appear, to which this Discourse formed the introduction, than the freedom which marked some of the opinions delivered, perhaps the omission of certain subjects altogether, but certainly much more than either of these circumstances, the well-known sentiments upon religious questions of many contributors, though that subject was in general avoided with care, raised a great opposition among the friends of the Church, who were soon joined by those of the temporal government; and this hostility was encouraged by all who made a trade of literature, the professed authors not belonging to the circle of the Encyclopædists, a name soon applied not only to the authors of the work but to the whole *free-thinking* part of the community. The storm soon became general, but the article 'Genève' was the first cause of attack. The free constitution of that little republic was praised, the conduct of its magistrates commended, the character of its people extolled, but there were doubts thrown upon the orthodoxy of its pastors, and a distinct condemnation was pronounced of Calvin's prohibition of the drama being still maintained in force.

Rousseau, though himself the author of plays and operas, attacked this article. His 'Letter' had extraordinary success, and D'Alembert's reply is on all hands allowed to have been a failure. Even his indiscriminate panegyrist, Condorcet, is fain to confess "Nous

avouons sans peine que sa réponse eut moins de succès." ('Hist. Ac.' 1783, p. 102.) The attack on the Encyclopædists was not confined to their literary adversaries or rivals, terms far too frequently synonymous, to the disgrace of letters. The circles of fashion, which at Paris always had their factious divisions, and always connected themselves both with literature and the theatre, took their share in the controversy. The clergy, of course, were not slow to join; and the Government became influenced against the great work and its conductors. D'Alembert now first knew what it was to have the hitherto unruffled calm of a geometrician's life broken and agitated by the tempests of controversy and of faction. Though he had never lived retired from the world, yet he had not been so mixed up in its affairs as to have acquired the callousness by which practical men soon become protected against the buffetings of the world. He could not easily reconcile himself to the bitterness that assailed him, and the injustice to which it led. When the Government refused in 1758 to let the 'Encyclopédie' be any longer published in France, and its seat was transferred to Neufchâtel, he retired from all share in the direction, (which Diderot alone continued to exercise,) and only contributed articles on mathematical and metaphysical subjects.

During the stormy years which now passed over his head he published his 'Mélanges de Philosophie, d'Histoire, et de Littérature,' his 'Memoirs of Queen Christina of Sweden,' his 'History of the Fall of the Jesuits,' and his 'Essay on the Intercourse of Literary Men with the Great,' a work in which he reads to his brethren lessons of independence, fully as distasteful as wholesome. His serious, rational, and dignified remonstrances are known to have at least had the salutary effect of terminating the degrading practice of authors dedicating their works, both of fancy and of science, to the great, in addresses which savoured rather

of prostrate submission before a superior being, than of gratitude for human patronage. He had long before accommodated his own practice to the course which his principles, as expounded in this Essay, would sanction; his first work (the 'Dynamique') having been inscribed to M. de Maurepas, Minister of Marine, in a respectful but dignified address, only stating that a scientific work was naturally enough dedicated to a statesman who protected the sciences.*

The annoyance and frequent irritation which the deviations from his proper pursuits occasioned him, made him always most willing to resume his more calm and congenial occupation. His researches on various important questions of physical astronomy, and his completion of the solution which he had a few years before given, as we have seen, of the great problem of disturbing forces, were published during the stormy years of his life. But it is truly painful to think that the soreness which he experienced from unjust attacks was suffered on more than one occasion to extend its influence into the serene regions of abstract science, and that the geometrician and the controversialist were sometimes perceived to be the same individual. The absurd attempt of ignorant men to depreciate his labours in the great problem, by representing him as borrowing from Clairaut, instead of only exciting his indignation against the silly propagators of such insinuations, which assuredly had no countenance whatever from Clairaut, as we have already seen, led him to show more heat than beseemed the geometrical character in scientific disputes on the subject

* His dedication to M. D'Argenson of his 'Essai sur la Résistance des Fluides,' did not by any means conform to his principles. After praising many other qualities, he ascribes, perhaps with some show of justice, to that virtuous Minister, "Modestie, candeur, amour du bien public, et toutes les vertus que notre siècle se contente d'estimer." Did he mean to conceal under the latter branch of this sentence only the meaning that M. D'Argenson gives an example of loving the virtues which others only admired?

with that illustrious colleague, whom he showed an unworthy disposition to differ with. A controversy of some length arose between them, when the principles of the solution respecting the lunar orbit were applied to the construction of lunar tables. D'Alembert's were published in his 'Recherches' in 1754, and he soon found their inaccuracy to be considerable; the results of his calculations sometimes differing seven or eight minutes from the observations. He was obliged in 1756 to give a corrected set after further investigation. Clairaut was writing at the same time on this subject, and he had received a prize from the Academy of Petersburgh for his work. D'Alembert, who had been a candidate too, attacked his methods in his 'Recherches,' 1756. Clairaut gave a criticism of this book and of the author's method in the 'Journal de Sçavans;' D'Alembert replied in the 'Mercure;' and Clairaut rejoined in 1758. The same unworthy spirit broke out on Clairaut having applied his investigation of the disturbing forces to the comet of 1682, (Halley's comet,) expected in 1759, but appearing a month earlier than Clairaut foretold, owing to an error of nineteen days in the computation. Anonymous attacks upon him he ascribed to D'Alembert, and a long series of controversial papers in different journals ensued; until Clairaut appeared to silence his adversary by an elaborate summary of the dispute, in 1762.* Again, when Clairaut

* I observe that Montucla (vol. iv. p. 72) considers D'Alembert as the author of the anonymous attacks, but he is evidently prejudiced against him. Indeed it is not clear that the editor, Lalande, may not have modified some passages. A person who could write the note about Clairaut might, indeed, be rather suspected of leaning against him. But there is no being certain respecting one who is so weak as Lalande; one who, not content with constantly recording his own small exploits in science, prints a motto under his portrait in the edition of Montucla, purporting that though the heavens were under his empire, and his genius penetrated through space, he yet reigned still more in the hearts of men. His flippant note (vol. iv. p. 188), on Boscovich shows his dislike of D'Alembert. "Le Père Boscovich ne fesait pas autant de calcul intégral que D'Alembert, mais il avoit bien autant d'esprit." He charges D'Alembert with perse-

investigated the figure of the earth upon the hypothesis of a variable density in the different zones, but the same throughout each, D'Alembert was not satisfied with giving his own solution more generally and more rigorously, but assailed Clairaut's hypothesis. However, this controversy was carried on with much less heat than the former. Geometricians appear to be agreed that in the one case, that of the lunar tables, Clairaut had the decided advantage over his adversary, whose mind did not easily lend itself to such details; but that the balance inclined in his favour upon the question of the earth's figure, D'Alembert's solution being certainly more general and less dependent upon assumption. His treatise on this subject is universally admired by geometricians, and it contains both the differential equations, then first given, of the equilibrium of fluids, and the new and most important theorem upon the relation between the polar oblateness and increase of gravita-

cuting the Père all his life. But little reliance can be placed on this assertion, at least if we may judge by the manifest falsehood of his statement, that "D'Alembert attacked Boscovich in his 'Opuscule,' vol. i. p. 246;" for all the attack consists in defending himself against an objection made by "an Italian geometrician of note in the science." The utter incompetency of a person like Lalande to edit such a work as Montucla's, can hardly be conceived without reading what he has done. Such ignorance or want of judgment is inconceivable, as could make him call Priestley's 'History of Optics' (so he terms it) a work of great importance, and one of its author's best, while by speaking of it as a book in 813 4to pages, he shows that he never had seen it; such ignorance as could also make him speak of Priestley's "universal erudition," vol. iii. p. 604, 5.) The entire want of common care as to dates is shown in his quoting Black's experiments as published in 1777 instead of 1755, if indeed this be a mere error, for the error is made to support the absurd argument that Priestley by his experiments, 1772, preceded Black as founder of the new chemical system. But nothing can be worse than Lalande's edition. The analytical expressions so abound with errors, possibly of the press, but which Lalande was incapable of correcting, that nothing can be more unsatisfactory than reading the book; nothing more tiresome than using the formulas, and finding, after perhaps a laborious investigation, as has happened to myself, that there was a gross error in them. Lalande's great merits, in his own department, both as a writer and a professor, beside his labours as a practical astronomer, stand wholly apart from his labours as an editor of a work in its main branches above him.

tion on all possible suppositions of the earth's internal structure. Finally, as regards this controversy, so painful to every reflecting geometrician, all men must be satisfied that in point of courtesy and candour there is no comparison between the two combatants. D'Alembert's blunt habits, which were excused in society as marks of simplicity, gave an unpleasant tinge of bitterness to his controversial writings, wholly unworthy of a philosopher, and little to be expected and less to be excused on questions of pure mathematics.

Let us, for relief from the pain which this portion of D'Alembert's history gives, do, as he did in the actual circumstances, retreat to geometry for comfort and for calm. In the midst of the virulent attacks which his 'Mélanges' called forth, and which were at the bottom of his soreness towards Clairaut on very different topics, see how he himself describes the truly philosophical course which his better reason indicated, and which he generally pursued: "Me voilà claquemuré" (walled in, or built round,) "pour long temps et vraisemblement pour toujours dans ma triste, ma très chere, et très paisible géometrie. Je suis fort content de trouver une prétexte pour ne plus rien faire dans le déchaînement que mon livre a excité contre moi. Je n'ai pourtant attaqué personne, ni même désigné qui que ce soit plus que n'a fait l'auteur du Méchant, et vingt autres contre lesquels personne ne s'est déchainé. Mais il y a heur et malheur. Je n'ai besoin ni de l'amitié de tous ces gens-là, puisque assurement je ne veux rien leur demander, ni de leur excuse, puisque j'ai bien résolu de ne jamais vivre avec eux: aussi je les mets à pis faire" (to do their worst). Again he says: "Eh bien! vous ne voulez pas, ni Fourmont non plus, que je me claquemure dans ma géometrie! J'en suis pourtant bien tenté! si vous saviez combien cette géometrie est une retraite douce à la paresse! et puis les sots ne vous lisent point, et par conséquent ne vous blâment, ni vous louent; et comptez-vous cet

avantage là pour rien? En tout cas j'ai de la géométrie pour un an tout au moins. Ah! que je fais à présent de belles choses que personne ne lira! J'ai bien quelques morceaux de littérature à traiter qui seroient peut-être assez agréables, mais je chasse tout cela de ma tête comme mauvais train. La géométrie est ma femme et je me suis remis en menage. Avec cela j'ai plus d'argent devant moi que je n'en puis dépenser. Ma foi, on est bien fou de se tant connoître par des choses qui ne rendent pas plus heureux; on a bien plutôt fait de dire 'Ne pourrois-je pas me passer de cela?' Et c'est la recette dont j'use depuis long temps."

It is to be considered that the abundance of income which he thus speaks of was not much above one hundred a year; for we know from himself that a short time before he had but 1700fs., or 68*l*., and the place of Pensionnaire Surnuméraire, which he obtained by election of the Academy in 1756, when he thus stated his means of living, could not have exceeded 1000fs.

In the autumn of 1752, the King of Prussia, to whom he had inscribed his Prize Memoir on the Winds, with some tolerable Latin lines,* invited him to settle in Berlin, offering a pension of 500*l*. a year, apartments and a table in the palace, with the office of President of the Academy, in the event of Maupertuis' death, who was not expected to live. D'Alembert refused this handsome offer, on the ground of his whole enjoyment being the society of his friends in the Parisian circle to which he belonged; and of his somewhat excessive fear of any connection which should interfere with, or put in jeopardy, the perfect freedom so essential to his happiness—a feeling so strong in him, that his friends used to say he was "the slave of his own liberty." At this time he states, in the correspondence

* Hæc ego de Ventis, dum Ventorum ocyor alis
Palantes agit Austriacos Fredericus, et orbi,
Insignis lauro, ramum protendit olivæ.

with M. D'Argens, through whom Frederick's offer was made, his income, as I have stated, did not exceed 1700fs.—not quite 70*l.* a year. The scruple of delicacy which he felt as to Maupertuis was at once removed by the King desiring him to take the appointments independent of all connection with the Academy, and assuring him that Maupertuis' wish was to have him for a successor. But nothing could tempt him to quit Paris. Ten years after this, he received a still more flattering offer, and one which, to an ambitious mind, would have presented more charms. The Empress of Russia, in 1762, desired him to undertake the superintendence of her son's education—the Czarowitch, afterwards the Emperor Paul. The appointments were £4000 a year, with residence in the palace. But still he preferred Paris, "the air of which agreed with his tastes and habits, notwithstanding the intolerance he was exposed to."

Indeed a great change had taken place in his manner of life, before either the Prussian monarch or the Russian became suitors for his favour. The society in which he now lived was one to which he had, about the year 1744, been introduced, and of which he soon became an intimate and esteemed member. It frequented the two houses of Mdme. Geoffrin and Mdme. du Deffand, or rather the house of the former, and the apartment which the latter occupied in the Convent of St. Joseph. Mdme. Geoffrin had succeeded to the coterie which used to assemble round Mdme. du Tencin, D'Alembert's mother; and all accounts agree in representing her as a person of extraordinary merit —sensible, clever, exceedingly amiable, of kindly disposition, and of the most active, but unostentatious benevolence. His intimacy continued to her death; or rather, as we shall presently see, to the commencement of her long illness. Mdme. du Deffand was a woman of another caste—very clever, extremely satirical, extremely selfish, and of a cold unamiable charac-

ter. Beside meeting his literary friends at her apartment, he there made an acquaintance which proved the bane of his life.

Mdlle. de l'Espinasse was a young person of great brilliancy, and of a warm and romantic disposition, which contributed as much as her talents to captivate all who came within the sphere of her attraction. The similarity of their history produced a mutual interest between her and D'Alembert, for she too was an illegitimate child. She was the daughter of Mdme. D'Albon, but not by her husband, being the fruit of a criminal intercourse with her lover. Mdme. D'Albon's daughter by her husband was married to M. de Vichy, and she allowed her unfortunate sister to live with her as a governess, her parents having only settled twelve pounds a year upon her. Constant ill usage in this house made her willing to accept the offer of Mdme. du Deffand, whose deceased husband was supposed to be her father. The moderate sum of sixteen pounds a year was to be allowed her; and in 1752 she went to live with her new patroness. Her humble office was to be the companion of that lady, to bear her intolerable humours, and to read her to sleep at an early hour of the morning—for in her life the night was turned into day, and she seldom rose much before sunset, or went to sleep before sunrise. The unhappy attendant was thus condemned also to pass her day in bed; but she rose an hour or two before her patroness, and that short interval, her only enjoyment of life, was passed in receiving D'Alembert and a few other friends, unknown to the Marchioness, who, however, discovered these secret meetings, and treating them as a conspiracy against her, drove the poor girl rudely from her situation, warning D'Alembert, at the same time, that he must choose between the two. As might be expected, he at once preferred his young friend; and, joining with others, obtained for her both a suitable residence and a small pension. An inflamma-

tion of the bowels, with which he very soon after was seized, and which had well nigh proved fatal, made it necessary, by the opinion of his physicians, to remove from his old nurse's small and ill-aired lodgings in the dark and narrow street, Rue Michel-le-Comte, in which, as in one of his letters he tells Voltaire, he only could see a yard or two of sky; and he took up his abode with Mdlle. de l'Espinasse, who had nursed him tenderly during his illness. No one whispered a syllable of suspicion respecting a connection which all were fully convinced could only be of the most innocent kind; and he continued to reside in the same apartment during the remaining twelve years of her singular life. It is now necessary to state some particulars of this attachment, which appear to have been given in an authentic form, and which cannot be easily reconciled with the feelings of a high and honourable nature, according to the facts as they stand recorded under his own hand.

Marmontel, one of the circle (côterie), and an intimate and admiring friend of D'Alembert, informs us that this young lady began to entertain the design of fixing in the substantial and regular form of wedded love, or at least of matrimony, the hitherto erratic admiration of which she had long been the object with many friends. He mentions an accomplished officer, M. Guibert, known for his able military writings, as the one on whom she first set her affections; and when he escaped her, tells us that she transferred her attempts to the Marquis Mora, a young Spanish grandee of the Fuentes family. But he falls into an evident mistake; for the correspondence of Mdlle. de l'Espinasse, since published, shows that she fell desperately in love with Guibert while she was carrying on her affair with Mora. Guibert, more wary and more experienced, avoided the snare. The Spaniard was completely caught; and being ordered home by his family, fell ill, as was said, from the excess of his

passion. She obtained an opinion of Lorry, the famous physician, that the air of France was necessary for his recovery; and his family yielding to this representation, he set out for Paris, but died on the way. Notwithstanding her passion for Guibert, which had been intercalated as it were, she is said to have taken Mora's death so much to heart, that her excitable and feeble frame could not stand against the shock, and she died about two years after, in May 1776.

Now, strange as it must seem to all men of right and honourable feelings, D'Alembert was so completely the dupe of his passion for her, that she made him the confidant of hers for Mora. Nay, he was sent every morning to the post-office for his absent and favoured rival's letters, that he might have them ready on her awakening. Nay, further, the opinion of Lorry which recalled him, was obtained through the solicitation of D'Alembert, the Doctor's intimate friend; and he wrote the most tender letter to Mora's father, condoling upon the young man's death. Marmontel sets all this down to the account of his extreme devotion to his mistress, and the great simplicity of his character. But this assumes that he believed her to be really in love with Mora. D'Alembert's own account is entirely different. In his 'Address to her Manes,' and his 'Address at her Tomb,' we find him distinctly complaining that she had deceived him, and made him believe for eight years and upwards that she loved him, when he discovered, by a paper left for him to read after her death, that all the time she really loved another. She appointed him her executor; and he found that she had kept masses of letters from others and not one from himself; also she bequeathed all these letters to different persons and none to him. He then bursts out into this complaint:—"Pourquoi les devoirs que cette exécution m'imposoit m'ont-ils appris, ce que je ne devois pas savoir et ce que j'aurois désiré ignorer? Pourquoi ne m'avez-vous pas ordonné brûler sans l'ouvrir ce manuscrit

funeste, que j'ai cru pouvoir lire sans y trouver de nouveaux sujets de douleur, et qui m'apprit que depuis huit ans au moins, je n'étois plus le premier objet de votre cœur, malgré toute l'assurance que vous m'en aviez si souvent donnée?"—He then goes on naturally enough to ask what security he could have, after this discovery, that she *ever* had loved him; and that she had not been also playing upon his affections ("trompé ma tendresse") during the eight or ten other years which he had believed to be so filled with love for him. (Œuv., Vol. I., p. 25.)

Now, how can we possibly account for this but by supposing, that she had made him believe her professed affection for Mora was all a pretence? But if so, what did he think was the nature of her connexion with that enthusiastic young Spaniard? Assuredly he must have been aware that Mora was in love with her. Then what was her plan with respect to him? I confess I am driven, how reluctantly soever, to the painful conclusion, that he lent himself to the plan of her inveigling the Spaniard into a marriage, and deceived himself into a belief that her heart was still his own. Marmontel's account is inaccurate enough in some particulars; but the story of D'Alembert's going for the young man's letters cannot be a fiction. It is an office no one could have easily invented for a lover. Besides, the apparent passion for Mora was known to all Mdlle. de l'Espinasse's circle. She never could conceal such a feeling when it took possession of her. That passion was not an affair of a few weeks or months; it lasted considerably more than six years; for in April, 1768, we find D'Alembert introducing him to Voltaire as his dear friend, and the young man's death was in May, 1774. (Corr. avec Voltaire, Œuv. XVI., 49.)

The fancy of this susceptible lady for Guibert was equally well known. D'Alembert saw these demonstrations of love as well as every one else; but she continued to make him believe that they were not real

indications of passion. This he tells us plainly himself. It remains to explain what he took them for; and no one can easily suppose that he was not made to believe they were connected with a plan of obtaining for her a settlement in life by marriage. The certificate which he obtained from Lorry to make Mora revisit Paris is of itself a proof that such was the project, and that to this project D'Alembert was privy.

The character of Mdlle. de l'Espinasse has been drawn by several masters, and by all in very favourable colours. Marmontel and D'Alembert himself have both laboured the portrait exceedingly; and if the passion of the latter may make the truth of the resemblance doubtful, at least to the pencil of the former, both more skilful and more faithful, we must give credit.—"Cette demoiselle étoit un éton nant composé de bienséance, de raison, de sagesse, avec la tête la plus vive, l'âme la plus ardente, l'imagination la plus inflammable, qui ait existé depuis Sappho. Le feu qui circuloit dans ses veines et dans ses nerfs, et qui donnoit à son esprit tant d'activité et de charme, l'a consommée avant le tems. Sa partie dans ces dînés (at Mdme. Geoffrin's, where she was the only woman present except the hostess,) étoit d'un intérêt inexprimable. Continuel objet d'attraction, soit qu'elle écoutât, soit qu'elle parlât elle-même, et personne ne parloit mieux; sans coquetterie, elle inspiroit l'innocent désir de lui plaire; sans pruderie, elle fesoit sentir à la liberté des propos jusqu'où elle pouvoit aller sans inquiéter la pudeur et sans effleurer la décence. Son talent de jeter en avant la pensée et de la donner à debattre à des hommes de cette classe (les Turgot, les Condillac, les D'Alembert, auprès d'elle comme un simple et docile enfant,) son talent de discuter elle-même, et comme eux avec précision, quelque fois avec éloquence; son talent d'amasser des nouvelles idées et de varier l'entretien, toujours avec l'aisance et la facilité d'une fée qui, d'un coup de baguette change à son

gré la scène de ses enchantemens; ce talent n'étoit d'une femme vulgaire. Ce n'etoit pas avec les niaiseries de la mode et de la vanité que tous les jours durant quatre heures de conversation, sans langueur et sans vide, elle savoit se rendre intéressante pour un cercle de bons esprits." (Marmontel, Vol. II.)

In the society of this attractive person, D'Alembert's evenings were all passed; and during the twelve years that elapsed between her quarrel with Madame du Duffand and her decease, he lived more constantly, of course, in her company, as he occupied the same lodgings. His mornings, after he quitted his study, were generally spent at Madame Geoffrin's; and the circle which he met at both those houses was nearly the same, except that Madame Geoffrin's was accessible to the better class of statesmen, according to her maxim that the protection of her favourites—the men of letters and of science—was well worth purchasing at this price; but for this use to which her benevolence knew how to turn them, she declared that after nine o'clock none but men of genius should find her door open, as far as her own taste was concerned.

The habits of French society, so entirely unlike our own, assemble in very small numbers, the same persons almost every evening at the same houses. The master or the mistress, generally the latter, hardly ever leaves home at the hours consecrated to this refined and agreeable intercourse, or only does so on stated nights, seldom more than one in a week. It is not easy for those who have never experienced the charms of this kind of society to understand its merits. Far from becoming dull or monotonous, in consequence of the sameness of the persons who compose it, this very circumstance it is that gives so much comfort and even enjoyment to the intercourse. The intimacy of a family circle is kept up, and the interest which each takes in the others becomes a powerful incentive to bestowing mutual confidence, while it gives a pleasurable feeling

to such as have no families of their own. There is, too, a variety always occurring, which no family circle can possess. The knowledge of each other's character, habits, pursuits, tastes, renders the conversation easy and interesting. The same subjects are continued from day to day. The kind of wit or humour of the circle is well known, and gives a zest to trifles, or sallies of pleasantry, that would be little relished by strangers. Add to which, that the familiarity of all with one another, though giving all a considerable interest in the welfare of each, stops short of inspiring so great an interest as would too much excite the feelings; and in this *quasi* family circle none of the anxiety is felt which often becomes too painful in the real domestic relations. The national character is, perhaps, better suited to such habits than ours would be. Certain it is that our neighbours consider us as having nothing which can be, with any propriety of speech, called society; for those whose lives are spent in coteries, when not occupied with business, regard with unmitigated aversion the large parties which, on rare occasions, bring together hundreds of their countrymen at some of our fair country-women's houses, and would have joined a late chief-justice* in his description of the obstruction which such assembled multitudes occasion of our streets, if his lordship, passing through the outer-door, had extended his definition of a nuisance to the scenes which pass within the walls of those fashionable and not inhospitable mansions.

All accounts agree in describing D'Alembert as a most agreeable and most acceptable member of those circles, first at Madame du Duffand's, and afterwards at Mademoiselle de l'Espinasse's and Madame Geoffrin's. His wit was very playful and easy, and it was without a particle of gall, though not unaccompanied with traits of satire, from which his writings are en-

* Lord Ellenborough.

tirely free. He is described as coming into society from his geometry like a boy escaped from school; and with the buoyant spirits which he drew from the success of his morning's investigations, combined with the pleasure of his present relaxation—a pure mind, free from all passions, satisfied with itself—a gentle and equal spirit, ever true, ever simple and natural, far removed from both pride and dissimulation—such is the picture drawn of him by the Marmontels, the Grimms, and the Diderots, who knew him best. His conversation was admitted to be delightful by the members of the most delightful and most fastidious circle in the world. His favourite maxim contributed to the charm of his conversation; he held that men should be most careful what they did, less careful about what they wrote, and least careful about what they spoke—a maxim to which he acted up in all respects himself. His inexhaustible memory—his lively unexpected sallies that never went a hair's-breadth too far—his inimitable talent of telling, and even of acting a story—his constant vein of liberal and enlightened, but sound, and therefore tolerant philosophy,—are the themes of those who survived him, and found that the blank he had left could not be filled up. That he possessed higher qualities than these is certain, for he was the most kind and charitable of men. Half his small income was given away in beneficent acts as soon as it became greater than his few wants and strict economy required. His patronage was easily obtained for merit; not easily, or at all, by powerful solicitation. An instance, and a celebrated one, occurred of this difference. When Laplace came to Paris as a young man, he brought letters of introduction to him from persons of importance in his native town; but no notice being taken of these, he wrote him a letter on the principles of mechanics. This produced an immediate invitation to call upon the Secretary, who told him he had no need of any introduction but his own merits, and in a

week obtained for him a professorship in the Ecole Militaire.

We have seen the warmth of his attachment to the object of his love. It remains to note the dreadful grief in which he was plunged by her death. Marmontel, whose tender friendship endeavoured to soothe his affliction, describes it as excessive:—"He seemed, in returning home to his apartment in the Louvre, as if he was burying himself in a tomb." But nothing better paints his affectionate nature, and the depth of his sorrow, than his own simple and touching expressions. Speaking, in a letter to Diderot, of the loss he had sustained already, and the impending one of Madame Geoffrin, he says,—"Je passois toutes mes soirées chez l'amie que j'avais perdue, et toutes mes matinées avec celle qui me reste encore. Je ne l'ai plus et il n'y a plus pour moi, ni soir ni matin." (Cor., Œuv., XIV. 250.) Madame Geoffrin was then on her death-bed, having for some months been given over. It was a great addition to his grief for Mademoiselle de l'Espinasse, that he was prevented from ever seeing the only person who could have offered him any consolation; but during the year that she lingered, her doors were barred against him by the cruel fanaticism of her daughter, whose name deserves to be recorded in order that her memory may be rescued from its apparent obscurity, and delivered over to the scorn of all good men, all charitable Christians. Madame de la Ferte-Imbaut thought fit to write him an insolent and intolerant letter, filled with abuse, and announcing that she took upon herself to deprive her dying parent of what must have proved a great comfort—the society of the man she most esteemed. The ground taken by this furious bigot was the known scepticism of the philosopher's opinions, though every one is aware that he never obtruded them on any society, and never gave to the world a single line in which religion and its institutions were treated with disrespect.

In the deep grief with which these irreparable losses struck him, his friends hastened to administer such consolation as their sympathy could afford. Among others, Frederick II. wrote him several letters, which are superior in point of feeling, and at least equal in ability, to any other of his works; and by that monarch's wise advice he was guided, and with success; for the only real relief which he experienced was in his favourite pursuit, his fast friend in good and in evil fortune, as Frederick advised him, (Œuv. Vol. XIII. p. 267.) He plunged into the depths of geometrical investigation, which he had too long abandoned, and he found the most salutary effects from this exertion. (Œuv. Vol. XVIII. p. 95.)*

The change which took place in D'Alembert's habits, when he became a member of those circles to which we have been referring, and passed in them no little portion of his time and all his leisure, may be supposed to have disinclined him towards his studious occupations, if it did not unfit him for them. But this was not the case. He had a great love of these pursuits, and a remarkable facility in following them; and the principal alteration which took place in his studies was, that he no longer confined himself to the mathematics, but undertook those other works of which mention has already been made. When he was chosen to succeed Duclos, in 1772, as Secretary to the Academy, the further labour devolved upon him of writing the Eloges of deceased members; and not content with this, he undertook to give the Eloges of those who had died between 1700 and 1772, and had not been commemo-

* It must be added as a sufficient reason for our regarding the affair of Mdlle. de Espinasse in the light of a publicly avowed matter, and not one only belonging to the immediate parties, that D'Alembert himself printed the letter to Count Fuentes on Mora's death, and also allowed Frederick's letters on Mdlle. de l'Espinasse's death to be copied, circulated, and published. Frederick was exceedingly offended with this; it produced a serious dryness, which lasted some time. (Vol. XVIII. p. 143. 155.)

rated by his predecessors. In three years he composed no fewer than seventy such biographical sketches, which, with thirteen others of his writing, fill six volumes of his works. Nor can we avoid feeling great regret that he should have wasted so much time and labour on a species of composition extremely little to be esteemed. For these Eloges are almost always remarkable for omitting whatever truths tell to the disadvantage of their subjects, so that they are of little value as history; and they are so slight and superficial as notices, that beyond giving dates and facts they give nothing. D'Alembert's offer no exception to this description; they do not record the history of the learned men's works of whose lives they profess to be sketches, and only general sketches. Many of them, indeed, relate to exceedingly obscure individuals, and the most distinguished are treated of in a manner quite unsatisfactory. The most elaborate is that of Boileau, in the notes of which we find a great number of literary anecdotes. The best, perhaps, is that of a man with no pretensions to literature, Lord Mareschall (Keith) because it contains a number of racy and characteristic traits of the worthy old politician. The taste and judgment shown in several is of a very equivocal character. Thus Massillon is described with some reference to his finer sermons, but very indifferent passages are selected for illustrating his prodigious merits; and his funeral sermons are plainly undervalued, without any exception being made in favour of the most magnificent passage, and the most successful that was perhaps ever delivered from the pulpit, the opening of the sermon on Louis le Grand's death.* Bossuet is plainly preferred to him; and some passages are given as master-pieces that are far exceeded by others in that great preacher's discourses. The "article" on the Abbé Dubois is enter-

* The body was lying in the church when Massillon began, "Dieu seul est grand, mes frères!"

taining; but, as if to show the incurable vices of the Eloge, a memoir being inserted written by one who had access to know the Abbé's history, D'Alembert admits his having suppressed those portions which reflected discredit upon him. It is necessary to add that the Eloges which D'Alembert composed officially as Secretary were, according to the custom of the Academy, read at the general or public meetings, which are attended by all who can obtain tickets of admission from the Academicians. At the same meetings were read other pieces of a popular description, as the 'Dialogue between Queen Christina and Descartes in Elysium,' that between 'Philosophy and Poetry,' and the 'Discourses on Poetry,' on 'Eloquence,' and others, upon the annual distribution of the prizes. That D'Alembert suffered himself to be seduced by the comparatively poor and passing gratification of pleasing or amusing promiscuous audiences on those occasions, cannot be doubted. The productions are of very ordinary merit. The two dialogues just referred to contain in their more solid portions nothing at all original or felicitous; and as jeux d'esprit, they may justly be said to have little of either playfulness or wit. The one in which Christina is a prolocutor, was delivered on the reception of Gustavus III. as a visitor, and it contains some singularly unmerited compliments* to that worthless and profligate prince, nowise distinguished either for their happy turn or the cautious procedure ever to be used in noting the merits of sovereigns too young to have shown how far taking them on trust is safe. Another jeu d'esprit, the 'Apology for Study,' is admitted among the warmest of D'Alembert's admirers to be a signal failure.

* "Sa modestie, ou plutot, et ce qui vaut bien mieux encore, sa simplicité, car la modestie est quelquefois hypocrite, et la simplicité ne l'est jamais." (IV. 82.) It would certainly have been difficult to find a word less applicable than *simplicité* to the subject of this flattery. See Statesmen of George III.'s time.

Another work of D'Alembert's, though not on a scientific subject, falls not within the remarks now made, his 'History of the Destruction of the Jesuits,' an important measure which had been finally accomplished by the Edict of the 6th of August, 1762, after their commercial speculations in Martinico had involved them in bankruptcy even prior to the capture of the island; and they had lost important law-suits with the mercantile interest in the Parliament of Paris. The Edict of 1762 was found insufficient to prevent the Society's subtle intrigues; and it was followed by several others, which dispersed them and forbade them to come within ten leagues of the capital. This work of D'Alembert, the 'History,' is only remarkable for its calmness and impartiality. He gives the amplest praise to the dispersed body, and allows them to be alone, of all the monastic orders, distinguished for their genius as well as learning, while of the others the only ones not sunk in ignorance were the Mendicant orders and the Benedictine; the former of whom were only scholastic writers, the latter literary compilers. He also shows that the Jansenists, the implacable enemies of the Jesuits, were exposed to great censure, and had acted like rigorous persecutors; and he takes the sound and rational course of maintaining that the destruction of one order could only be defended on principles which lead to the destruction of all other orders of monks, and in every state. In other respects the merit of the 'History' is but moderate. There is nothing very happy in the narrative, which, indeed, is unconnected, and has the worst of historical faults, proceeding by way of allusion more frequently than of plain and direct recital. There is nothing very original or profound in the remarks. There is nothing striking in the descriptions. The style has the excellent qualities of all D'Alembert's writings, clearness and simplicity, and this is the principal praise to which the work is entitled.

His translation of select passages of Tacitus, executed with great zeal, as might be expected from his exaggerated admiration of that classic, and the kind of delusion respecting him under which he laboured, is certainly much better than his critical opinion on the original. But his ideas of a translator's duties are singularly incorrect. He complains of the common run of translators for being so "superstitiously attached to their authors, that they fear to embellish them even in feeble passages;" and contends, by a ridiculous sophism, that as we must often fall short of the originals, so we ought to take compensation by surpassing them when we can. He tells us that he sketched his translations with much rapidity to avoid coldness, and afterwards corrected with great care—a proceeding not perhaps much to be condemned; but he adds, that he has occasionally taken the liberty of altering the meaning when "the Latin presented a puerile image or idea, and when Tacitus appears to be belowhimself." (Ib. 26.) Thus he lends Tacitus a little wit, a metaphor, indeed, in the celebrated description of Tiberius, whose dislike alike of freedom and of flattery made men's words difficult and slippery, or perilous. "Angusta et lubrica oratio sub principe qui libertatem metuebat, adulationem oderat." (Ann. 11, 87.) "Tant la servitude même marchoit par une route étroite et glissante, sous un prince qui detestoit la flatterie et craignait la liberté." (Œuv. xiv. 167.) Can any one doubt that this is a total perversion of the sense? Tacitus does not say, nor could he with truth say, that the one noble quality of the crafty but able tyrant, his detestation of flattery, made all the actions of men slippery and doubtful. He knew well that in every other respect submissive obedience was their only care; but the dislike of flattery only created some doubt when they were to write or to speak. Accordingly, other translators have preserved the sense of the original without losing the fine and picturesque expression of "Angusta et lubrica." "Rien

de plus étroit et de plus glissant que l'usage de la parole," says La Bletterie. "Aussi ne restoit-il à l'eloquence qu'un sentier étroit et bien glissant," says another. I have dwelt upon this passage because it is a special favourite of the author, who gives four pages of commentary on his version. So in the famous passage on Domitian, the highly wrought diction and vivid imagery of Tacitus is not sufficient to satisfy the translator. "Præcipua miseriarum pars erat videre et aspici; cum suspiria subscriberentur; cum denotandis tot hominum palloribus sufficeret sævus ille vultus et rubor quo se contra pudorem muniebat." ('Vit. Ag.' c. XLV.) "La fureur de Domitien était plus cruelle que les supplices même; nos soupirs etoient comptés; et le visage du tyran, inflammé par le crime et inaccessible à la honte, rendit plus touchante la pâleur du tant de mourans." (xiii. 267.) It is not too much to say that D'Alembert, with all his admiration of Tacitus, thought he had greatly improved upon him; though while affirming that his author had lost "nothing by the translation," he candidly admits "that the original is at least as fine." (Cor. Part. Œuv. xiv., 392.*) It is, however, now admitted by all critics that a good translation of Tacitus into any modern language is impossible I remember Dr. Parr once saying, in answer to a learned person who asked, or rather took the liberty of asking, his opinion which was the best translation of Tacitus, —"Sir, I thought every one had long since admitted there can be none."

Among D'Alembert's other writings of the inferior kind, to which I have been referring, must be reckoned his 'General Reflections on Eloquence.' They are superficial and inaccurate, though, like most of his

* Numberless examples of failures could easily be given; but I have only selected a few to show the consequences of his absurd theory of translation. In the character of the Fenni (De Mor. Germ.) "Fennis mirâ feritas, fœda paupertas," D'Alembert renders this most tamely and most imperfectly, "très-féroces et très-pauvres:" thus getting rid entirely of the sense of the Latin (xiii. 238.)

literary pieces, somewhat dogmatical with their shallowness. His very definition of Eloquence is entirely faulty; he calls it the faculty of communicating to others the feelings that fill our own minds; according to which, however dull or impotent these feelings may be, their impression being truly conveyed, they produce all the effects of the highest eloquence, and so every person may be eloquent, nay, almost all may be equally eloquent. His reflections on History are of no higher merit. Of his notions respecting Poetry we have already spoken.

It remains to speak of his general treatise on the 'Elements of Philosophy.' It is one of his best literary works, and certainly preferable to that which it approaches nearest in the subject-matter, the Introductory Discourse to the Encyclopædie. It is exceedingly comprehensive; it is rapid without being hurried or hasty; it is as clearly written as possible; and it is accompanied with illustrations judiciously given and very convenient for the general reader. But though it be well entitled to these commendations, it is not easy to follow Condorcet in his eulogy of this piece as containing an important "metaphysical discovery." He regards it as settling for the first time the controversy "whether the laws of motion belong to the class of contingent or of necessary truths," and he considers D'Alembert as having first discovered the demonstration that these laws are necessary. Now nothing can be more certain than that D'Alembert does no such thing as prove this position. He only shows, what never could be doubted, that the deductions from certain assumed facts are necessary and not contingent. Assuming the existence of matter, and also its impenetrability, he treats the vis inertiæ as demonstrated, and also its corollary, the uniformity of motion once begun and not affected by any external causes. But the impenetrability of matter is a contingent truth as well as its existence; and there is nothing in the definition of

matter or of motion to make it impossible that a motion once begun should cease at a time proportioned for example to its quickness, or should be accelerated by the very nature of the original impulse: and so of the equality of action and reaction. No doubt, if the vis inertiæ be granted, and the equality of action and reaction, the composition of forces may be demonstrated, and so may the proposition of equal areas in equal times, and the principle of equilibrium first discovered by D'Alembert. But these are only mathematical demonstrations of truths deducible and issuing from contingent truths. The propositions of geometry are wholly different; they result necessarily from the definitions; they are indeed involved in those definitions. Thus, if a circle is defined as the curve described by the extremity of a given straight line revolving round a fixed point, in this definition there is really contained the proposition that its length is proportional to the describing line's length, and its surface to the square of that line. We affirm in these two propositions only that if there be a curve line such as to have all the lines equal, which are drawn to it from a given point, that curve must have a certain measure of its length and surface. When we affirm that a body moves in the diagonal if solicited by two impulses along the two sides of a parallelogram, we assume, not merely that there is a body and that there is motion, but that the body has certain qualities and that motion has certain laws, and these are facts which exist, not mere suppositions which we make. D'Alembert has only the merit, and a great one it is, of having, first in his 'Dynamique,' and afterwards in his 'Élémens,' reduced the whole laws of motion and equilibrium to the fewest and simplest possible fundamental principles, and therefore generalized those principles.

All D'Alembert's writings have now passed under our review: it remains to form a more general estimate of his merits in the two capacities with a detailed

view of which we have been occupied, his merits as a man of science and a man of letters. And certainly the difference is very wide between his position in these two different classes; nor can I avoid marvelling, with Sir J. Mackintosh, at the partiality which so far blinded Mr. Stewart, as to make him consider him very eminent in both.

Among mathematicians he holds a high place indeed, ranking on the very first line. Euler was perhaps a more fertile analyst; and he gave incomparably greater contributions to the science, than either D'Alembert or indeed any other man. Clairaut was excelled by none in the profoundness of his researches, and the originality of his methods, and he passed all others in the marvellous precocity of his genius as a geometrician. At the same time, we can never forget that D'Alembert's discovery of the dynamical theorem, and his most felicitous employment of it to arrange the whole of mechanical science, exceeds anything accomplished by either of his illustrious contemporaries in usefulness, indeed in originality; while of a most important calculus he was, if not the father, certainly the person who by applying it and teaching its uses, effected a great change in the face of geometrical and physical science. His investigation of the lunar orbit; of the earth's figure, of the precession and the nutation, would have entitled him to rank with Euler and with Clairaut, and before Fontaine, had his 'Dynamique' and his 'Partial Differences'* never been given to the world. On the latter subject, Euler and Fontaine in some sort anticipated him; but taking the former discovery into our account, and his application of the calculus, we shall probably be justified in placing him the first among the philosophers and geometricians who succeeded Sir Isaac Newton.

* It is in his two works on Fluids, and in his Memoirs on the Winds and Vibrating Chords, that we find this method, and rather used or applied than explained.

It is equally clear that no comparison can be instituted between him and that most illustrious of the human race. The 'Principia' stands at an immeasurable distance before the 'Dynamique;' and the Calculus of Partial Differences is but an improvement, though a very great one, of the Method of Fluxions; while the optical discoveries of Newton have so little that can be compared with them in the history we are contemplating, that D'Alembert never could bring himself to take an interest at all in experimental philosophy, much less to make any discoveries for extending its bounds. Not only was he without any pretension of this kind, but he was incapacitated from such pursuits by his entire ignorance of many branches of physical science, an ignorance almost general with him on everything which did not lend itself to geometry or rather analysis,—an ignorance, be it further observed, extremely discreditable to his understanding as a philosopher. Who can read without astonishment his avowal that he knows nothing of chemistry; an avowal borne out by some of his writings, and by the Discourse to the 'Encyclopédie;' when we reflect at the same time that the greatest of geometricians and analysts did not disdain to be as thoroughly acquainted with the chemistry of his age, as any one who knew nothing else? Indeed some of his most wonderful conjectures respecting the constituent parts of bodies, may be referred as much to chemical as to optical science.*

D'Alembert's reason for undervaluing the truths of inductive philosophy, must be allowed to have been wholly unworthy of his genius for general speculation. He thought meanly of the evidence on which it rests, and could take no interest in any investigations other than analytical. Can any one doubt that the evidence of experiments is in the highest degree deserving of our respectful attention, without refusing also his ap-

* See especially the Queries to the 'Optics.' I remember Dr. Black citing these wonderful productions with unbounded admiration.

proval to the whole of human conduct, which of necessity proceeds upon the admission that contingent truths, both physical and moral, rest on sufficient grounds for us safely to act upon them in all the affairs of life? Besides, D'Alembert admitted, both in theory and by his own conduct, that physical science was deserving of attention, when it could bear the application of the calculus. Then how could he be sure that any given branch of experimental philosophy might not be susceptible of strictly mathematical treatment, unless he made himself master of that branch? We find Cavendish applying geometrical and analytical reasoning to such subjects as electricity. We have profound Memoirs of my illustrious and lamented colleague, M. Poisson, treating the same subject by the resources of the calculus of which he was so great a master. Capillary attraction received a similar consideration from Laplace; analysis has been successfully applied to optical researches by mathematicians of our own times. But I would not by any means be understood in these observations to admit that purely inductive researches, and those to which no geometrical reasoning can be applied, are less worthy of a philosopher's regard than those which easily ally themselves with the science of necessary truth. No one who has studied the inimitable experimental investigations of the second book of the 'Optics,' can hesitate in admitting that they are in every way worthy of the immortal author of the 'Principia.' The inquiries of Black and Cavendish excite the like admiration. Nay, has not D'Alembert himself written many profound optical papers? We have some of these in the 1st, 5th, and 7th volumes of the 'Opuscules,' and the 3rd volume is composed wholly of such. How then could he tell beforehand that he might not find other physical subjects capable of geometrical treatment?

It remains to note the inferiority in point of elegance in D'Alembert's investigations to those of many other geometricians. He was anxious only for the result;

and the truth once discovered, he was extremely indifferent to the neatness of the investigation, whether of the steps by which the analysis had guided his course, or of the synthetical deduction by which he demonstrated the proposition. His own observation was, "Let us discover truths, and there will never want those who can put them in shape." Possibly his quickness (or facilité) the only quality beside "some talent,"* which he modestly claimed for himself, may have had its share in producing this carelessness about any elaboration of his analysis. He is generally clear enough in his explanations, always logical in his reasonings; but we enjoy not the pleasure of seeing the truth unfolded by the most striking methods, or traced in its most surprising relations and connected by remarkable analogies with kindred matters.

If, from contemplating the eminent merits of this illustrious geometrician, we turn to regard him in his literary capacity, there is, unquestionably, a signal falling off. He cannot be said here to occupy even a second place. It is to be observed, that his entering upon the belles-lettres, and, indeed, upon moral and historical subjects also, was a deviation from his original, and, as it were, his appointed course; nor ought the failures of great men ever to be visited with censure, but under the influence of this candid and just consideration. The accidental relations of society first seduced him from geometry, and the appointment of Secretary to the Academy completed the desertion of his mistress, leading him to indulge in the meretricious course of delivering popular essays to promiscuous assemblies on great occasions of academical display. To the task of handling literary subjects, too, he came with a most imperfect preparation. He had no depth at all of learning; his knowledge of Latin was respect-

* "Il a apporté dans l'étude de la haute géométrie, quelque talent et beaucoup de facilité; ce qui lui a fait un assez grand nom de très-bonne heure." Portrait par lui-même. (Œuv. i. xliv.)

able, not extensive or profound; of Greek very far from considerable, indeed hardly competent; and of the principles of criticism he was imperfectly master. In truth nothing could be more alien to his natural and amiable diffidence than the position which he assumed, without any title whatever, of dictating *ex cathedrâ* his many crude opinions and hasty and superficial comments on literary topics. His taste, accordingly, as a critic, was, without being positively vicious, certainly far from very correct. He appears to have preferred Bossuet to Massillon; but in this he agrees with probably the majority of his countrymen. He is far from placing Corneille on the same level to which his powerful genius has by general consent elevated him; and his pleasure was great when he found the idol of his worship, Voltaire, joining in repeated attempts to decry that illustrious author. Even Racine pleases him but little. The versification he thinks a model, but the dramatic effect small. 'Athalie' is a "Tragédie de collège" without action, without interest. He compares Racine, Boileau, and Voltaire, together thus; Boileau makes us think and feel what labour the verse has cost: Racine makes us think without feeling it: Voltaire makes us neither think it nor feel it; and to him he gives the decided preference. (Cor. de Volt., Œuv. xvi. 106.)

Indeed, Voltaire was in all things his idol. No one can read any of his literary works and not be convinced that he regarded that extraordinary man as standing at the head of all writers, ancient and modern, upon literary subjects, as well as of all poets. The first impression made upon him was, in all probability, by Voltaire's dramatic works. His other poems confirmed and extended the influence thus acquired over his mind; and the sceptical opinions and satirical spirit of his prose writings completed the enchantment, leaving him no power of supposing either that the god of his idolatry could ever err, or that anything was beyond his reach

—insomuch that we actually find him infinitely flattered "par le suffrage accordé à l'article 'Géométrie,'" and hoping that Voltaire would be equally pleased with the articles on Forces and Gravitation, and begging him to read that on the Figure of the Earth, the merit of which consists in his correcting Clairaut's hypothesis, and on this correction Voltaire was utterly incapable of offering an opinion. The article on Gravitation consists of four sections, three of which are full of calculus, and so unintelligible to Voltaire that it seemed like a mockery to mention them. (Cor. de Volt., Œuv., xv., 41.)

The admiration which he expresses for Tasso is certainly quite legitimate. But who can allow him to single the 'Gerusalemme' out of all ancient and modern epics, as the "only one which we can read from beginning to end with pleasure and interest?" (Œuv. iv., 116.) He had just pronounced, dogmatically, the somewhat astounding dictum, that no one can read Virgil or Homer through without being weary of the task. When he singles out Tasso, indeed, he makes him the solitary exception "among dead poets;" but this qualification is manifestly introduced on behalf of the 'Henriade,' the author of which was still alive.

It is another proof of defective taste that he admires Tacitus beyond all the writers of antiquity, which critics of a much less severe taste than D'Alembert have not been tasteless enough to do. "Préjugé de traducteur à part (says he) comme il est sans comparaison le plus grand historien de l'antiquité, il est aussi celui dont il y a le plus à recueiller." He goes on to speak of the "various kinds of beauty of which this incomparable writer gives the model," and after mentioning "the energy of his descriptions of men, and the pathos of his narrative of events," ends with this astounding assertion, "qu'il possède dans un si haut degré la véritable éloquence, le talent de dire simplement de grandes choses." (Œuv. vii., 23.) I own that when I first

read this passage I looked to see if there might not have been omitted, by an error of the press, the words "quoique" and "ne pas." It is hardly credible that any one should have singled out for commendation in Tacitus the very quality which he notoriously possesses not. We find the same enthusiastic admiration breaking out in his correspondence: "Quel homme que ce Tacite!" (Cor. Part., Œuv. xiv., 332.) We find him, too, consoling his afflictions with the writings of that historian, whom he quotes in both the letters addressed to Diderot on Mde. Geoffrin's death. (Cor. Part., Œuv. xiv., 251, 261.)

But it is not only from defective taste and insufficient knowledge, that D'Alembert's literary works fall so immeasurably below his scientific. They are in general, extremely slight and superficial. His capacity of deep thought no where appears. There is sufficient calmness in the tone of the remarks; the discussions, when he does discuss, are conducted with commendable impartiality, and the sentiments are generally those of a liberal, enlightened and unprejudiced mind; but no force is put forth; no difficulty is grappled with; nothing original or striking appears in the views taken; nothing very felicitous in the illustrations; nothing profound in the argument. The "great facility," or quickness, which has been already noted as characterizing his geometrical capacity, had a fatal effect when he deviated into lighter studies; it lulled his attention asleep and prevented the severe labour which great works in the belles-lettres demand, as in every other department of human exertion. All his writings are more or less slight and insufficient. By far the most elaborate are, the Discourse in the 'Encyclopédie' and the 'Elements of Philosophy;' but the first of these must be confessed to fail from the radical defect of its fundamental principles; and the second, though superior, does not rise much above mediocrity, nor leave on the mind any lively or lasting impression.

Of the style in which all his writings are composed, the great merit must at once be admitted. It has the good quality of perfect clearness and of undeviating simplicity. The taste which it displays is very far superior to what could have been expected from so warm an admirer of Tacitus. It seems as if his other passion, that which devoted him to Voltaire, together with his keen sense of ridicule, had effectually saved him from the rock upon which the admirers of Tacitus have so generally made shipwreck, and had purged his diction of those false ornaments in which men of science are so very apt to indulge when they quit their proper haunts and descend into the low but perilous sphere of fine writing. Would that our physical, ay, and even our geometrical writers would always keep the great example of D'Alembert before their eyes—not only when they deviate from their proper orbit into general speculation, but even when they are confined to their own subjects! How much vile figure and inaccurate trope; how many jumbled metaphors, disjointed declamations, and misplaced quotations, should we then be spared! His own character of his style is not at all too favourable, exemplifying what it describes: "Son style serré, clair et précis, ordinairement facile, sans prétension quoique châtie, quelquefois un peu sec, mais jamais de mauvais goût, a plus d'énergie que de chaleur, plus de justesse que d'imagination, plus de noblesse que de grace."*

We have now surveyed this illustrious life in its various phases, and observed its merits reduced to their real, but still magnificent dimensions. The events by which it was diversified were necessarily few. The kind of existence which D'Alembert enjoyed in his study and the society of Paris has been described. From those habits he seldom deviated, unless in so far as his whole literary occupations may be considered

* Portrait de lui-même. (Œuv. i. xlv.)

to have been, what indeed they were, a deviation. His intercourse with Voltaire and with Frederick II. have been mentioned, and it was nearly all that can be said to have variegated the tranquil and uniform tenor of his way.

To Voltaire at Ferney he paid a visit in the autumn of 1756; and it is plain from all Voltaire's letters that this occurrence gave the greatest satisfaction to "the Patriarch." The tenor of their correspondence was one of uninterrupted confidence and mutual esteem. That D'Alembert occasionally sacrificed somewhat of his wonted independence to his profound admiration of his friend, is certain. A mathematician like him should never have given to Voltaire's ignorant and ridiculous assertion that Leibnitz and Descartes were two charlatans ('Corr. Vol.' Œuv., XVI. 77) so tame a reply as merely to say, that he had not read the collection of Leibnitz' works, but readily believed it to be "un fatras où il y a bien peu de choses à apprendre" (Ib., 80). Though Voltaire may only have spoken of that great man's universality, an objection which it little becomes either himself or his correspondent to make, yet the first geometrician of the age ought never to have left the subject without a protest in favour of the founder of modern Analysis. There is, however, something very touching in the ease with which D'Alembert bowed before the errors and the ignorance of genius, contrasted with the sturdiness of his resistance to all the attempts of mere station or private friendship to influence his opinion. Mdme. du Deffand, then the patroness of his mistress and his own, in vain besought him to slide in a word on behalf of her friend the President Hénault when the 'Discours' was preparing. D'Alembert peremptorily refused to say one syllable of that feeble and correct chronologer in the 'Discours,' and would only, under the head of "Chronology," go so far as to say, that he had written one of the three chronological abridg-

ments which were useful, but not the best of them ('Œuv.,' XIV., 322, 343).

The correspondence with Frederick II. was continued for thirty years, during three-and-twenty of which it was constant and regular. There is, perhaps, as much independence in it on the philosopher's part as can well be expected in such circumstances; yet, certainly, a very considerable portion is filled with constantly-repeated expressions of respect, devotion, gratitude, and of admiration for the royal qualities and station. The letters written on any days that happened to be anniversaries of Frederick's victories, are always dated "Anniversary of such and such a battle" (see XVII., 16, 422, &c. &c.) A Frenchman, whose country was at war with Frederick, expresses his joy at all that prince's victories for six years, except only the one over the French army at Rosbach (XVII. 7.) A scornful opinion of his intimate friend Diderot's works, and a report as contemptuous of his personal qualities (XVII. 381), is only met with a prediction that, should his Majesty see Diderot, he would judge more favourably of him than he had done of his works (Ib., 383.) Flattery, of course, is lavished unsparingly. Not only is Frederick the Cæsar of the age, which he certainly might fairly be termed, but he is raised to a divine rank, being commemorated as both Mars and Apollo (Ib., 259, 389.) Nor is any clear expression of opinion given, when after committing the greatest public crime in modern times—the partition of Poland—Frederick sent the philosopher his Polish Medal, with the false motto, "Regno reintegrato." He coolly takes it as a proof that the King had only taken the step of re-entering into the possession of his own old dominions (XVII. 329); and after the lapse of eight years had left no possible doubt on the nature of the transaction, we find him introducing Ruhlières to the King as desirous of writing Polish History under his patronage, and expressing "his great admiration of his

Majesty." But the wary King-partitioner had the sense to see what might follow from hence, and told his correspondent that the event was too recent to be the fit subject of an historical work (XVII. 235, 6. 240.)

In the course of this correspondence D'Alembert went twice to visit Frederick,—once in 1755, when the latter was at Wesel on the Rhine; and again in 1763, when he passed two months with the king at Potsdam. The impression left on the royal mind by both these visits was highly favourable to D'Alembert, as might well be expected from his modest, ingenuous nature, and excellent social habits.

Towards his sixty-fourth year his health—which had never been robust, though his life was eminently temperate, and always with an entire abstinence from fermented liquors—began to decline. A feeble digestion and constant difficulty of sleeping, had long been the bane of his bodily comfort. To these ailments was now added an affection of the bladder, which his medical friends found to be beyond the reach of their art. He suffered exceedingly for the last three years of his life, and suffered with an exemplary calmness and even cheerfulness: at length, exhausted with pain, with irritation more than pain, with sleeplessness, with indigestion, and its consequent weakness, he expired on the twenty ninth of October, 1783, in the sixty-seventh year of his age. His most intimate friend, Diderot, died of dropsy nearly about the same time. It is emphatically stated by Grimm, whose intimacy with Diderot gave him means of knowing the truth of the assertion, that D'Alembert might have prolonged his life had he not refused submitting to a surgical operation. Be that as it may, during his long and painful illness his mind appeared exhausted like his body, but the mental feebleness was only apparent; for the intervals of ease which he had were occupied with mathematical investigations, and with other subjects that interested him. His sick chamber was

attended by numerous friends, among whom he alone retained his gaiety, enlivening the conversation with sallies of pleasantry, in which their feelings would hardly let them participate. Condorcet was, he knew, to write his éloge for both Academies. A day or two before his death he said to him, "Mon ami, vous ferez mon éloge dans les deux Académies, vous n'avez pas de tems à perdre pour cette double besogne." ('Grim. Corr.') Yet sometimes the torment he endured overpowered him; and his unostentatious dislike of all pretence, all acting, prevented him from concealing his agony. "Nature," said he, "has left a suffering being the relief of complaining." And if he ever accused himself of importunately afflicting his friends by his sufferings, he would say that he could hardly "conceive how so feeble a creature was able to endure so much without dying." The certainty of his end approaching was announced to him, and he received the tidings with the most absolute tranquillity. His cheerfulness remained unbroken; and the last words he uttered were to a friend who attended his deathbed: "Do you hear how my chest is filling?" M. Pouque, member of the Institute, communicated this interesting anecdote to La Harpe. The words were addressed to him.

The fame which D'Alembert for a long course of years enjoyed all over Europe, was certainly greater than that of any other man of science in any age. Voltaire's was little or nothing among philosophers; and prodigious though the reputation always was of the poet and literary man, his opinions upon religious subjects were so generally known, indeed so openly declared, that his reputation, how great soever, was to a certain degree of a party caste. D'Alembert, the first philosopher of the age, was likewise advantageously known among literary men, and estimated above his deserts in letters on account of his admitted superiority in science. During his life, too, though attached

to the party of the Free Thinkers by his habits in society, he had never made himself obnoxious by any public declaration of his opinions; and was indeed never publicly known to be an infidel till his correspondence with Voltaire appeared after his death. There was no name, therefore which carried such weight among men as his; and while he lived, though cabals among politicians now and then interfered against him, as when his academical pension was delayed, because, in a letter opened at the post-office, he was found to have called the Duc de Choiseul, Voltaire's protégé rather than his protector; yet in general, full justice was done to his transcendent merits, and his name was everywhere amply honoured. A letter of Abbé Galliani may be cited as showing the estimation he was held in even at Naples, where one might have expected merit, such as his, would be slow to penetrate. The Abbé thus gaily refers to a letter some one had brought from the great man:—"Elle m'est si chère, me cause tant de plaisir, me rend si glorieux, que c'est le meilleur présent que j'eusse pu recevoir de Paris. Si vous voyiez comme je me rengorge endisant dans la compagnie, '*Je viens de recevoir une lettre de D'Alembert,*'—que je tire à moitié de ma poche, et que j'y laisse tomber sans en faire la lecture à cause d'un certain petit bricole qu'il y a dedans, qui n'est pas pour tout le monde." I cannot refrain from continuing the quotation of this truly witty letter:—"Sur cela grands discours sur D'Alembert; grand étonnement lorsque je dis qu'il est petit de taille, pantomime et polisson au possible. On veut partout que vous soyez grand comme St. Christophe, et sérieux et barbeux comme le Moïse de Michel Ange.* On finit par me demander tous à la fois, 'L'avez vous-vu?' comme on demandait a Pape Panurge dans l'Ile des Papegais et des Papefigues. Non, en vérité, un Mes-

* I have corrected the manifest error of the books which make it "Moine."

sinois n'est pas si vain de sa lettre de la Madonne que je le suis de la vôtre." (Œuv., XIV., 399.) Such is the style of one who himself stood at the very head of the most witty and agreeable society of the times; and was more run after than any one of its members. And it may safely be affirmed that no man in any circle of Europe, would in those days (1773) have received a letter from D'Alembert with different emotions.*

The neutrality which he had always during his life maintained upon sacred subjects, was unfortunately confined to his published writings; and a few years only elapsed after his decease, before the real state of his religious opinions became well known by the publication of Voltaire's correspondence and Frederick II.'s The fame which his reputation had hitherto enjoyed, caused a great and general reaction among the zealous friends of the Church, a reaction proportioned to the tolerance previously exercised towards him, while men were in the dark respecting his opinions. Nevertheless nothing could be more unjust or unreflecting than the indignation which thus broke forth. He had studiously avoided all offence, whatever opportunity he might have had of giving it. A very pious and even zealous writer, while giving vent to his strong feelings on religion, has the candour to condemn the want of charity shown towards D'Alembert on this subject, declaring that his infidelity was only "a fault God-ward, and which men had no right to visit with censure, because he never published one phrase of an irreligious tendency, while his writings contain many warm expressions in favour of Christianity and its professors." (Portrait de D'Alembert, Œuv. I. lxvii.) This testi-

* This letter is one of the most charming for its light gay wit, that is any where to be found; nothing can give a higher idea of the Abbe's powers. The profound sense of it is on a par with the wit. Thus:—"La crainte et l'avidité sont et seront toujours les causes de la cruauté:" which he proceeds to illustrate by a most picturesque allusion to the conduct of the most cruel of men—the Spaniards in America.

mony from a writer who cries out against the 'Encyclopédie,' as "an arsenal of irreligion," dispenses with the necessity of adding proofs to show how fairly and even kindly D'Alembert ever talked of Christianity in public. But another and a more reverend authority may be cited to the same effect. M. Coetloquest, Bishop of Limoges, said that he had never seen him, but that he had always heard that his morals were above reproach; and his Lordship added, "Quant à ses ouvrages je les lis souvent, et je n'y trouve que beaucoup d'esprit, de grandes lumières, et une bonne morale. S'il ne pense pas aussi bien qu'il écrit, il faudroit le plaindre; mais personne n'est en droit d'interroger sa conscience." The detestation which D'Alembert expresses, even in his private letters, of the 'Système de la Nature,' (XLI. 371. XVII. 225,) may be cited with the same view, as may the horror of Atheism which he repeatedly testifies.* And if in reality he was a zealous adversary of religion, it has been justly observed by La Harpe, that his hostility was far more directed against its ministers than against the system itself.* Nor ought we even to express our condemnation of such conduct, or our regret for its injustice, which view soever we may take of this subject, without considering the extreme provocation which the French philosophers of that age had to endure. Calas, old and infirm, broken on the wheel as the murderer of his son, a robust young man, in the presence of many of his family, to prevent him from abjuring Catholicism; La Barre condemned to have his tongue cut out, and dying in agony, because while a boy he made faces at the procession of the priests; a poor creature condemned to the galleys and pillory, and dying of the fright the

* See especially in the Hist. de la Destruction des Jésuites, Œuv. v. 134. "Ce malheureux (l'athée) très-coupable aux yeux de Dieu et de raison, n'est nuisible qu'à lui-même." It is clear from all he says of the 'Système de la Nature,' that he never could have believed Diderot to be the author; perhaps not even D'Holbach.

day after the sentence, for having offered a bookseller a book which he knew nothing of and had received in payment of a debt:—these were the scenes that passed before the eyes of D'Alembert and Voltaire; nor let us, who have no such excuse for hating the establishment, visit too severely the sentiments which scenes like these not unnaturally raised in generous minds, how much soever we may be disposed to admit that they carried their indignation beyond just bounds when they confounded the use with the abuse, and made religion answerable for the faults of its professors.*

* The character given of D'Alembert by Grimm, is certainly more remarkable for its epigrammatic composition than its truth; though it may contain an approximation to some features. "Les personnes qui ont vécu le plus avec D'Alembert le trouvaient bon sans bonté, sensible sans sensibilité, vain sans orgueil, chagrin sans tristesse;" all this he explains by ascribing to him a combination of "roideur, faiblesse, et activité." He allows his conversation to have been admirable, that he could lend attraction to the most dry and forbidding subjects, and gave his sallies with a grace and a readiness not easily surpassed.

APPENDIX.

I.

NOTE ON D'ALEMBERT'S PRINCIPLE.

Professor Playfair ('Ed. Rev.' xi., 253) has by no means been happy in his enunciation of the Principle. "If the motions which the particles of a moving or a system of moving bodies have at any instant be resolved into each two, one of which is the motion which the particle had in the preceding instant, then the sum of all these third motions must be such that they are in equilibrium with one another."

The following are the observations referred to in p. 396, note.

The great utility of this principle proceeds from the universality of its operation, and from its supplying the place of the detached artifices and ingenious assumptions by which dynamical problems had hitherto been treated, by a rule directly applicable to the circumstances of the motion of one or more bodies whose motions were any other than those immediately proceeding from the direct and unfettered action of the motive force.

The principle applies equally to the most elementary and the most difficult problems—to the motion of a body down an inclined plane—the vibrations of a simple pendulum—or to the theory of the radiation of heat—the vibrations of a chord: two subjects previously of insuperable difficulty, to which the illustrious author applied his new method, and which became remarkable in his hands, not only for the solutions which he obtained, but also for the manner of them—for it was his singular good fortune, by a further invention, to overcome the analytical difficulties into which the fecundity of his dynamical principle had led him.

The great utility of this principle will not appear from the comparison of the solutions of any one problem obtained by its means, with the detached artifices previously employed;

these were all private paths to one solution, whilst that is a high road to all. The solution of every problem is obtained from an equation involving some principle to which the motions of the system are subject—the advantage of D'Alembert's step lay in this, that it was *the same* principle which he applied to each particular case.

Note to p. 396, line 19, by the author mentioned p. 396, note.

Since these last forces mutually destroy each other, and that the forces actually impressed were compounded of them and of those (usually called *effective*) which act in the direction the bodies really move in, so that the force originally applied (usually called the *impressed* force) is the result of these two forces, it follows that the *effective* forces would, if they acted in the contrary direction, exactly balance the *impressed* forces. Problems of dynamics are thus reduced to a general equation of equilibrium and become statical.

II.

That Euler, in the Memoir published in 1784, solved an equation of Partial Differences is quite incontestable, though he laid down no general method; which, indeed, D'Alembert himself never did, nor any geometrician before the publication of Euler's third vol. of the 'Institutions of the Integral and Differential Calculus.' The problem, as given in the 'Mem. Acad. Petersb.' vol. vii., was this; We have the equation $d z = \mathrm{P}\, d x + \mathrm{Q}\, d a$, z being a function of x and a; and the problem is to find the most general value of P and Q, which will satisfy the equation. $\mathrm{Q} = \mathrm{F}\, z + \mathrm{P}\,\mathrm{R}$, F being a function of a, and R a function of a and x, Euler seeks for the factor which will make $d x + \mathrm{R}\, d a$ integrable. Call this factor S, and make $\mathrm{S}\, d x + \mathrm{S}\,\mathrm{R}\, d a = d\,\mathrm{T}$, and make $\int \mathrm{F}\, d a = \log.\mathrm{B}$.

He finds for the values required

$$\mathrm{P} = \mathrm{B}\,\mathrm{S} f' : \mathrm{T},\ \mathrm{Q} = \frac{z\, d\,\mathrm{B}}{\mathrm{B}\, d a} + \mathrm{B}\,\mathrm{R}\,\mathrm{S} f : \mathrm{T}$$

and from thence he deduces

$$d z = \mathrm{B\,S}\,(d x + \mathrm{R}\,d a) f' : \mathrm{T} + z \frac{d\,\mathrm{B}}{\mathrm{B}} =$$

$$= \mathrm{B}\,d f : \mathrm{T} + z \frac{d\,\mathrm{B}}{\mathrm{B}} \text{ and}$$

consequently $z = \mathrm{B} f : \mathrm{T}$.

It is thus clear, that Euler had, in or before 1734, integrated an equation of Partial Differences; and it must further be remarked, that D'Alembert, in his paper on the Winds, the first application of the calculus, quotes Euler's paper of 1734. D'Alembert always differed with Euler respecting the extent to which this calculus can be applied, holding, contrary to Euler's opinion, that it does not include irregular and discontinuous arbitrary functions.*

III.

The Vitrière's house, in which D'Alembert was brought up and lived afterwards for so many years, can no longer be ascertained. I have examined this matter with some care in the street in which it stood, Rue Michel-le-Comte. That street is very narrow, in no place above eighteen or nineteen feet wide, and the houses on both sides are lofty. D'Alembert, therefore, did not exaggerate when, in his letter to Voltaire, he said he could only see a yard or two of the sky from his room. The street is near the Rue St. Martin, at some distance north of the Hôtel-de-Ville. The church of St. Jean-le-Rond, at the gate of which he was exposed, and from which he took his name, stood near the cathedral of Notre Dame, and was pulled down in 1748. It was a baptistery of Notre Dame, near the Foundling Hospital, and touched the Cathedral Church. Of the Vitrière's house I have inquired everywhere, not only in the Rue Michel-le-Comte, but at the Prefecture (Hôtel-de-Ville), and among my brethren of the Institute; I can discover no traces of it. D'Alembert's Address given on his admission to the Academy in 1741, only mentions the street without giving any number.

* Cousin has mentioned the anticipation of Euler. 'Astronomie, Disc. Prélim.'

ADDITIONAL APPENDIX

TO THE LIVES OF

SIR JOSEPH BANKS AND ADAM SMITH.

CAPT. COOK TO MR. BANKS.

"WILLS'S COFFEE-HOUSE, CHARING CROSS,
"Sunday Morning, [1768.]

"DEAR SIR,

"Your very obliging letter was the first messenger that conveyed to me Lord Sandwich's intentions. Promotion, unsolicited, to a man in my situation in life, must convey a satisfaction to the mind that is better conceived than described. I had this morning the honour to wait upon his Lordship, who renewed his promises to me, and in so obliging and polite a manner as convinced me he approved of the voyage. The reputation I may have acquired on this account, by which I shall receive promotion, calls to my mind the very great assistance I received therein from you, which will ever be remembered with most grateful acknowledgments by,

"Dear Sir,
"Your most obliged humble servant,
"JAMES COOK."

CAPT. COOK TO MR. BANKS.

"SIR, "SHEERNESS, 2nd June, 1772.

"I received your letter by one of your people, acquainting me that you had ordered everything belonging to you to be removed out of the ship, and desiring my assistance therein.

"I hope, Sir, you will find this done to your satisfaction, and with that care the present hurry and confused state of the ship required. Some few articles which were for the

mess I have kept, for which, together with the money I have remaining in my hands, I shall account with you for, when I come to Town.

"Taught by experience not to trust to the knowledge of servants the whole of every necessary article wanting in such a voyage, I had, independent of what I purchased for the mess, laid in a stock of most articles, which will be now quite sufficient for me, and is the reason why I have not kept more of yours.

"The cook and two French-horn men are at liberty to go whenever they please. Several of the casks your things are in belong to the King, are charged to me, and for which I must be accountable. I shall be much obliged to you to send them to the Victualling-Office when they are emptied, but desire that you will by no means put yourself to any ill-conveniency on this head, as I shall not be called upon to account for them until my return.

"If it should not be convenient to send down for what may be still remaining in the ship of yours, they shall be sent you by

"Sir,

"Your most obedient and very humble servant,

"James Cook."

"My best respects to the Doctor; and since I am not to have your company in the 'Resolution,' I most sincerely wish you success in all your exploring undertakings."

Capt. Cook to Mr. Banks.

"'Resolution,' Cape of Good Hope,

"Dear Sir, "18th Nov., 1772.

"Some cross circumstances which happened at the latter part of the equipment of the 'Resolution' created, I have reason to think, a coolness betwixt you and I; but I can by no means think it was sufficient to me to break off all correspondence with a man I am under many obligations to.

"I wish I had something interesting to communicate, but our passage here has rather been barren on that head. We touched at St. Jago, where we remained two days, and Mr. Forster got some things there new in your way. Mr.

Brand has got for you a fine collection, as I am told. I depart from hence in a day or two well stored with every necessary thing, but I am told the French from the Mauritius have got the start of me. About eight months ago two ships from that island discovered land in the latitude of 48 degrees, and about the meridian of the Mauritius, along which they sailed forty miles till they came to a bay, into which they were about to enter, when they were separated, and drove off the coast by a gale of wind. The one got to the Mauritius soon after, and the other is since arrived from Batavia with a cargo of arrack, as the report goes here; also, in March last, two frigates from the same island touched here in their way to the South Sea, having on board the man Bougainville brought from Otaheite, and who died before the ships departed hence, a circumstance I am really sorry for. These ships were to touch some where on the coast of America, and afterwards to proceed round Cape Horn.

"I am in your debt for the pickled and dried salmon which you left on board, which a little time ago was most excellent; but the eight casks of pickled salted fish I kept for myself proved so bad that even the hogs would not eat it. These hints may be of use to you in providing for your intended expedition, in which I wish you all the success you can wish yourself; and am, with great esteem and respect,

"Dear Sir,

"Your most obliged humble servant,

"James Cook."

Capt. Cook to Mr. Banks.

"Dear Sir, "Plymouth Sound, July 10th, 1776.

"As you was so obliging as to say you would give a description of the New Zealand spruce tree, or any other plant, the drawing of which might accompany my Journal, I desired Mr. Strahan and Mr. Stuart, who have the charge of the publication, to give you extracts out the manuscript of such descriptions as I had given (if any), for you to correct or describe yourself, as may be most agreeable. I know not what plates Mr. Forster may have got engraved of natural history that will come into my books; nor do I know

of any that will be of use to it, but the spruce tree and tea plant and scurvy grass; and I know not if this last is engraved. The flax plant is engraved: but whether the publishing of this in my Journal will be of any use to seamen I shall not determine. In short, whatever plates of this kind falls to my share, I shall hope for your kind assistance in giving some short account of them. On my arrival here I gave Omai three guineas, which sent him on shore in high spirits: indeed he could hardly be otherwise, for he is very much caressed here by every person of note; and, upon the whole, I think he rejoices at the prospect of going home.

"I now only wait for a wind to put to sea; unless Capt. Clarke makes good haste down, he will have to follow me. Sir John Pringle writes me that the Council of the Royal Society have decreed me the Prize Medal of this year. I am obliged to you and my other good friends for this unmerited honour.

"Omai joins his best respects to you and Dr. Solander with,

"Dear Sir,

"Your most obedient and very humble servant,

"JAMES COOK."

These letters are given out of their place, not having come into my possession until after the Life of Sir J. Banks was printed. They appeared sufficiently interesting to be here inserted. The same reason must justify the inserting, also out of its place, the following extract from a letter of Dr. Black to Mr. Smith, on his 'Wealth of Nations.' The rest of the letter regards Mr. Hume's health. It was written apparently in August, 1776. Nothing can be more interesting than to mark the sentiments of a great and original genius respecting the exertions of another and congenial spirit in a different walk of science.

DR. BLACK TO ADAM SMITH.

"Though I sit down to write to you upon another account, I cannot help expressing the pleasure and satisfaction

I frequently meet with in hearing the opinions of good judges concerning your book. I most heartily rejoice in the prospect of the additional credit and reputation which you cannot miss to gain by it, and which must increase as long as you live; for I have no doubt that the views you have given of many parts of your subject will be found by experience to be as just as they are new and interesting. And although it be admired immediately by discerning and impartial judges, it will require more time before others who are not so quick-sighted, or whose minds are warped by prejudice or interest, can understand and relish such a comprehensive system, framed with such just and liberal sentiments."

NOTE TO THE LIVES

OF

CAVENDISH, WATT, AND BLACK.

I HAD not read M. Cuvier's 'Eloge de M. Cavendish' when these Lives were first published. That Eloge is contained in the Memoirs of the Institute for 1811.* Its composition certainly justifies the title of Eloge; for it is a very indiscriminate, and not very accurate panegyric of an illustrious man, whose memory was best preserved and honoured by a correct statement of the facts. M. Cuvier makes no mention whatever of Watt in connection with the discovery of the composition of water. But he is not much more just to Black himself on that of fixed air; or, as he calls him, Blake: clearly showing that he had never taken the trouble even to look at any work of that great man. As to Mr. Cavendish, he gives it for part of his Eulogy that he explained his doctrines " dans une manière plus étonnante encore que leur découverte même," (p. cxxvi.) Now if M. Cuvier had read the paper upon the combustion of inflammable air, he certainly would have found that this remark in no respect whatever applies to it, for the composition of water is but darkly shadowed out in that celebrated Memoir.

He proceeds to say, that in 1766 Mr. Cavendish undertook, in his paper read before the Royal Society, to establish propositions, " presqu'inouies jusque là; l'eau n'est pas un élément; il existe plusieurs sortes d'airs, essentiellement

* Nothing can be more confused, more inconsistent, than the manner of publishing the volumes of this great work. It is generally a year or two and even three or four, after the real date of the papers; thus this twelfth volume of the new series is called 'Mem. année 1811,' yet it contains only two papers of 1810-11; all the rest were received in 1812-13. I have remarked this more fully in the Lives of Lavoisier and D'Alembert.

différentes." He then mentions Von Helmont inaccurately, as having ascertained that there were "permanently elastic vapours other than atmospheric air;" and Hales, still more inaccurately, as having measured these permanently elastic fluids: whereas Hales considered them all as common air, combined with impure exhalations,—an opinion which prevailed a century and a half after Von Helmont, and was adopted by D'Alembert in his article "Air" in the Encyclopædie, 1751. Black's discovery of fixed air, he confines solely to its explaining the causticity of the alkalis and earths. No one, he says, before Cavendish had distinguished it as a separate aëriform substance; and though emanations were said to proceed from bodies, no one knew in what they consisted. Cavendish, he says, first settled in 1766 all these questions, and showed that this air, whether from chalk or fermentation, or mines, was one and the same fluid, "auquel on a depuis reservé le nom d'air fixe." Finally he (Cavendish) discovered that it was the air evolved from burning charcoal (p. cxxx). He then ascribes the application of inflammable air to raising balloons in the air to M. Charles's application of Mr. Cavendish's experiments on the specific gravity of that gas.

This is really somewhat astounding. That a person of M. Cuvier's eminent attainments, filling the high office of Secrétaire Perpétuel, and charged with the delicate and important duty of recording the history of science yearly, should not have deemed it worth his while to read either the celebrated experiments on Magnesia Alba and Quicklime published in 1755, or the Lectures published in 1803, before assuming to write the history of chemical discovery, is wholly beyond belief. Had he read the former work he would have found that Dr. Black gave to the air which he had discovered the name of fixed air; and that he did so, not because it was the same with, or any modification of, atmospheric air, but simply because *air* was a known term in common use to represent a permanently elastic fluid, and because this kind of air was found *fixed* in combination with bodies. Had he looked at the Lectures, he would have found that two years after the publication of his capital discovery, viz., in 1757, and nine years before Mr. Cavendish's paper was received, Dr. Black discovered that fixed air is

the gas evolved in fermentation, and that he found it to be so by the very experiment now in use to show it, namely, emptying half of a phial filled with lime water in the air of a brewer's vat, when the remaining lime water becomes turbid, the carbonate of lime being formed and precipitated; that he discovered on the same day the identity of fixed air with that evolved from burning charcoal; and finally, that he also ascertained the air evolved from the lungs in respiration to be fixed air, by breathing through a syphon half filled with lime water. All this, which M. Cuvier ascribes to Mr. Cavendish's discoveries, in 1766, had been published by Dr. Black in 1755, and explained by the experiments themselves being performed by his own hands, in his public lectures, every year before nearly three hundred persons, from the year 1757 to the time of Mr. Cavendish's supposed discovery in 1766. Of these Lectures numberless copies were taken, were in general circulation, and were sold to the students attending the classes of the College in Edinburgh. It is, however, very possible that Mr. Cavendish was not apprised of Dr. Black's experiment made before 1752 and published in 1755. But it is quite certain that he never arrogated to himself the discovery of fixed air being a peculiar body different from common air, for he expressly says, "By fixed air I mean that peculiar species of factitious air which is separated from alkaline substances by solution in acids, or by calcination, and to which *Dr. Black has given that name* in his Treatise on Quicklime." ('Phil. Trans.,' LVI., p. 140.) Now this shows clearly that M. Cuvier never had read Mr. Cavendish's paper, any more than he had read Dr. Black's Treatise, and his Lectures. Another proof is his asserting that Mr. Cavendish discovered the air evolved from burning charcoal to be fixed air. His paper contains not one word on that air as connected with burning charcoal. Nay, so far is Mr. Cavendish from assuming to himself the discovery of its identity with the air evolved in fermentation, that he expressly says Dr. Macbride had discovered the evolving of fixed air in that process, and that he himself only made his experiments to ascertain if any other air was also evolved, when he found inflammable air also to come away. Apparently he had not been aware of Dr. Black's experiments in 1757. The Lectures would also

have shown M. Cuvier that Dr. Black, as early as 1766, showed his friends the ascent of a bladder filled with inflammable air, long before the experiments of M. Charles, to whom the earliest observation of this fact is by M. Cuvier rashly ascribed.

M. Cuvier mentions Macquer as having first observed the deposit of moisture when inflammable air is burnt. He says nothing of Mr. Warltire's experiment, though Mr. Cavendish himself states expressly ('Phil. Trans.' 1784, p. 126) that it was the deposit of dew observed by Warltire, which set him on making his experiments. From this omission of M. Cuvier, it is plain that he never took the trouble to read the paper of Mr. Cavendish, which, as he refers to it by volume and page, he may, therefore, have seen—he never could have read it. This also accounts for his singular assertion, that Mr. Cavendish's discoveries were explained with an evidence and a clearness more astonishing than the discoveries themselves.

It is equally incorrect to affirm, as M. Cuvier appears to do, p. cxxxiii., that the decomposition of water suggested by M. de la Place, and performed by M. Lavoisier, became "la clef de la voûte," for the analytical experiment is equivocal, and the synthetical alone is precise. He says that M. Monge had made the same experiments as Mr. Cavendish, and had the same idea, "*avoit eu la même idée,*" probably meaning that of a quantity of water being formed equal to the quantity of airs burnt, and had communicated the result to Lavoisier and La Place; and Monge seems really to give the first notion of water being composed of these airs, as La Place's; for he says, "Si la combustion de ces airs donne de l'eau, dit M. de la Place, c'est qu'ils résultent de sa décomposition." Had M. Cuvier really read the work he so often cites, the 'Philosophical Transactions,' he would have found Mr. Watt's letter, and he could hardly have avoided mentioning the first idea of the composition as his.

But truly it is to be lamented that the history of science should be written with such remarkable carelessness, and such manifest inattention to the facts. To find mistakes so very gross in the works of ordinary writers might excite little surprise; but when they are embodied in the history of the National Institute, and when they come to us under

the name, among the very first in all sciences, of Cuvier, we may at once wonder and mourn.

Since the Life of Watt was published, a very strange attack on both M. Arago and myself, but more especially on my illustrious colleague, has appeared in the 'Quarterly Review.' The ingenious and (as far as this controversy is concerned) not very learned critic appears to be led away by the excess of his zeal for Mr. Cavendish. I leave him in the hands of M. Arago, who will observe with some wonder that he has been accused and judged and condemned by a chemist so well versed in that science, and so reflecting, as to announce the astonishing novelty, that the exhibition of sulphur to sulphuric acid reduces that acid and restores it to its primitive state of sulphur! The writer had probably read somewhere that *sulphuric* acid is reduced to *sulphurous* by the process; for he is assuredly the first that had ever hit upon the acid's reduction by sulphur "to its primitive state."* I have lying before me fifteen pages of statements of chemical errors in the thirty-four pages of the paper; and as these are the work of a most experienced and learned and practical chemist, whom I consulted on the above and other parts of the paper, I have entire reliance on his report and opinion. I must also add that he completely bears out, by the authority of his concurring opinion, the statements (disputed by the critic) which I had ventured to make respecting Dr. Black's discoveries, with the single exception that he is not aware how far I am justified in stating the greater specific gravity of fixed air as known to him before Mr. Cavendish's experiments in 1766. My reason for so stating was my distinct recollection of Dr. B. having in his lectures shown us the experiment of pouring fixed air out of a receiver on a candle, and his having given this as a property originally known to himself when he discovered the gas, though it is very true that the published lectures do not decide either way the question of his early knowledge. His not mentioning Mr. Cavendish or any one

* The process of reducing phosphoric acid to its primitive phosphorus, had just been stated, and the writer adds, "A similar succession of phenomena are presented by sulphur, &c.;" and he enumerates sulphur as one of the bodies which reduce the acid to its primitive state.

else as having first taught it him is with me, who well knew his scrupulous exactness in such matters, quite decisive of his having himself observed it.

I shall only cite further my correspondent's note on the Reviewer's statement, "that I was wrong in ascribing to Dr. Black the discovery that fixed air has acid properties." (p. 110.)—"The Reviewer adds that 'the acidity of fixed air was indicated for the first time by Priestley and his fellow-labourers, and only completely established by Lavoisier, who showed fixed air to be carbonic acid, or a mixture of carbon and oxygen.' His Lordship is quite right, and the Reviewer doubly and egregiously wrong. Priestley did *not* indicate for the first time the acidity of fixed air. Whether he understood Black's views concerning it does not appear, but he expressly disclaims the discovery as his own. His words are, 'It is not improbable but that fixed air itself may be of the nature of an acid, though of a weak and peculiar sort. Mr. Bergman of Upsal, who honoured me with a letter upon the subject, calls it the aërial acid; and among other experiments to prove it to be an acid, he says that it changes the blue juice of tournesole into red. ('Phil. Trans.' 1772, vol. lxx., p. 153.) It does not appear whether Black was aware of the reddening action of fixed air on vegetable colours, but he was abundantly aware of the *functions* of fixed air as an acid; that is, of its power to neutralize bases, and to form salts by combination with them. Black's own words are, 'These considerations led me to conclude that the relation between fixed air and alkaline substances was somewhat similar to the relation between these and acids; that as the calcareous earths and alkalis attract acids strongly, and can be saturated with them, so they also attract fixed air, and are in their ordinary state saturated with it.' ('Experiments upon Magnesia Alba,' &c., p. 50.) The whole page might be quoted. Nothing could be more satisfactory to a chemist than this statement. The modern definition of an acid is 'a substance which neutralizes bases, and by combination with them, forms salts.' Power to affect vegetable colours, or sour taste, the vulgar attributes of an acid, are wanting in many of the most powerful of them: for example, in silicic acid. The Reviewer's reference to Lavoisier is quite meaningless. The French chemist showed

that fixed air was an oxide of carbon. Whether it was an acid oxide or not, could not be determined by analysis. That problem could be solved only by ascertaining whether or not it formed salts by combining with bases. That is the only method possible at the present day, and was the one Black followed."

So very easy is it for ill-informed and inaccurate writers to launch charges of ignorance and inaccuracy and carelessness against others! M. Arago will no doubt be fully sensible of this truth, though he will furnish no example of it in his own person or in his defence of himself.

As for the mysterious passage in p. 117, which states that the critic had prepared a commentary on my account of Mr. Cavendish's experiment regarding the density of the earth, but that, possibly through pity towards a fellow creature, he suppressed it, giving, however, as the result, that it would show "the most ingenious and entire distortion, not merely of nearly every step in the process itself, but of nearly every principle involved in it,"—I can only, with all humility, but with all comfort, mention, that the passage is none of my own, being taken very closely from the work of a most profound mathematician, professor of the science in one of our Universities; and that, in borrowing it, I find that I have avoided two errors in the original, one the misprint (apparently) of *friction* for *torsion*, the other the confining the comparison to the time of the oscillation, whereas I make it general, including therefore both the length and the duration. I wrote the account at a distance from Mr. Cavendish's paper, and therefore took it at second hand. If friction is intended, and not torsion, in the account which I copied, it is an omission certainly. How it can be called a distortion, I cannot comprehend, nor can the learned Professor himself, whom I have consulted. I say nothing of a similar charge respecting the Torricellian experiment, except to observe, that my reference to it is most studiously framed to exclude the very construction put upon it by the critic, as the sentence beginning "unless" must plainly show to any candid reader.

Now I write with great and unfeigned personal respect for the learned critic, who, had his work been given under the sanction of his name, would have been more careful in

all likelihood. But one discovery having been mentioned, I must add, that he also has made another, a discovery which, I think, would have surprised my friend Mr. Vernon Harcourt himself, as much as it did his other readers, "that there are very few amongst the most distinguished of our countrymen superior to" that reverend and excellent person, "either as a writer or as a man of science;" so great a length will zeal for his friend and fellow polemic carry a critic engaged in a controversy.

But this zeal is readily explained by the reflection that fellow-combatants in any controversy which heats their tempers, are blind to each other's deficiencies, and exaggerate each other's perfections; they are also prone to exaggerate the services rendered by each other to the common cause. "The unanswerable arguments of my noble, or my honourable friend," is a very familiar expression on every side in Parliamentary debates, which one thus finds are conducted on both sides by combatants equally invincible, and therefore ought always to prove drawn battles. So the critic holds Mr. Vernon Harcourt's publication from Mr. Cavendish's Journals, to be decisive in favour of his contention; whereas those extracts demonstrate, that Mr. Cavendish never had, even privately, given the explanation of his experiment until after Mr. Watt's theory was in the hands of the Royal Society. I am very far from arguing upon this important publication of Mr. Vernon Harcourt's, that Mr. Cavendish borrowed the hint from Mr. Watt; but at least it demonstrates that Mr. Watt had reduced his theory to writing before Mr. Cavendish, and could not by possibility have borrowed it from him.

It must once more be repeated, that I never charged or thought of charging Mr. Cavendish with having obtained from Mr. Watt's paper his knowledge of the composition of water, and having knowingly borrowed it, however suspicious a case Mr. Harcourt's publication may seem to make. Both those great men, in my opinion, made the discovery apart from each other, and ignorant each of the other's doctrine. Mr. Cavendish was a man of the strictest integrity, and the most perfect sense of justice. His feelings were very far inferior to his principles. He was singularly callous to the ordinary calls of humanity, as

there exist positive proofs sufficient to satisfy the polemical writer upon whose paper I have been commenting if he has any mind to see them. Nor do they rest on my assertion, for I never had any intercourse with him except in society. But the pursuits of a philosopher and the life of a recluse, which had so entirely hardened his heart, had not in the least degree impaired his sense of justice; and my own belief is, that he as entirely supposed himself to have alone made the discovery in question, as Sir Isaac Newton believed himself to be the sole discoverer of the nature of light, and the theory of the solar system.

Mr. J. Watt and M. Arago may now safely be left to carry on the controversy, whether with the reverend author, or with his able and ingenious, though somewhat over-zealous critic. The subject left in their hands is safe, and the truth is sure to prevail. In these circumstances I am far from feeling any anxiety as to the result, or any desire to anticipate the arguments and the statements which must so soon be brought forward. But as I have been freely and most rashly charged with inaccuracy, with inattention to facts, even with having omitted to read the original papers on which the question turns, and charged, in company with my friends M. Arago and Mr. J. Watt, (one of the most careful, laborious, and scrupulously exact of men,) I may simply assert, that as regards myself no imputation can well be more groundless; for there is not a single one of the whole papers which I have not repeatedly and sedulously examined, both alone and in company with others who took an interest in the controversy. I might add, that never was a charge made with a worse grace than this by the ingenious, and most careless, and very moderately-informed critic who has mixed in the discussion; for assuredly *he* has not taken the trouble to read the papers, or to make himself acquainted with the works which every chemist, even every student of chemistry familiarly knows. What shall we say of a writer who undertakes to discuss this question, with no better provision for handling it, than is betokened by his broadly affirming that Mr. Watt himself never preferred the disputed claim, when there exists his own paper of 1784 in the 'Philosophical Transactions,' referring to and indeed containing his letter of April, 1783? Nay, what

shall we again say of the same critic as broadly asserting, that no one ever in Mr. Cavendish's lifetime brought it forward, when Professor Robison in the Encyclopædia, Dr. John Thomson in his celebated Translation of Fourcroy, Dr. Thomas Thomson and Mr. Murray, each in their 'Elements of Chemistry,' and Mr. W. Nicholson in both his 'Dictionary' and his other works, all state Mr. Watt's claim in the very words in which M. Arago and myself now have urged it, nay, Sir C. Blagden states it in his letter to Crell, and all these long and long before Mr. Cavendish's death,* to say nothing of others, as Dr. Thomson, in his 'History of the Royal Society,' published since? As to Mr. Vernon Harcourt's appealing boldly to Dr. Henry's authority, and preserving a profound silence when I quoted his letter, expressly negativing that confident statement, I say nothing; because it is a matter not easily handled, consistently with the respect and esteem in which I have ever held my reverend friend.

* Professor Robison in 1797; the Translation of Fourcroy earlier.

NOTE TO THE LIFE OF SIMSON.

The remarkable circumstance of the case of the comet's motion, for which Sir I. Newton's solution was intended, proving to be the porismatic case of the construction, has been mentioned in the text. It has been sometimes considered as singular, that this did not occur to himself, the more especially as he evidently had observed two cases in which the problem became indeterminate—namely, when the lines were parallel, and when they all met in one point, for he excepts those cases in express terms (*Prin. lib.* 1. *Lem.* xxvii.) It may be observed, that such oversights could very rarely happen to the ancient geometers, because they most carefully examined each variation in the data, and so gave to their solutions such a fulness as exhausted the subject.

The commentators on the Principia (Le Seur and Jacquier) make no mention of the omission. The circumstance of the Porismatic case was not discovered till ten years after their publication, when F. Boscovich found it out, in 1749. But it is very extraordinary that Montucla appears to have been unaware of the matter, although the first edition of his work did not appear till 1758. Nor is the least reference made to it in the second edition, which was published the year he died (1799.) There are other omissions in both editions, and also in the continuation. He appears well to have understood the ancient method, and to have read and examined some of the most celebrated works upon it. He had given due praise to Simson in his first edition; and to Lord Stanhope, who sent him the 'Opera Reliqua;' and we find in the second edition a full note upon the subject, II. 277. In the continuation—III. 11, and seq., we have further indications of the attention which he had bestowed upon the ancient geometry;

but it is remarkable that though Matthew Stewart's Tracts, published in 1761, were known to him, he was wholly unacquainted with the 'Propositiones Geometricæ,' which appeared soon after, and with the General Theorems which had been published fifteen years before. Nor does he appear to have even seen Professor Playfair's admirable paper upon Porisms in the Edinburgh Transactions, 1794, the war having probably impeded the intercourse of the two countries. Had he seen this, he must have been brought acquainted with the history of the Porism relating to the Comet's place, for it is there fully given.

It must be added, that Montucla's mathematical pursuits had for many years been interrupted by the duties of the places which he held under the government, until the Revolution (Pref. 111); and although the loss of those employments restored him to his studies, it is probable that he rather applied himself to the continuation of the History, the bringing it down from the period to which the first volume extended, than to supply omissions in those volumes, considerable as are the additions which he made to them.

The third and fourth volumes were not published till after his death, which happened when only a third part of the former had been printed. Lalande undertook the revision of the rest, and how great soever his merits may have been as a practical astronomer, as an author, and a teacher of astronomy, he had none of the mathematical acquirements which could fit him for superintending the publication of Montucla's work. He had some assistance from a very eminent mathematician, Lacroix, and the notes given by him are, as might be expected, excellent. But we are not distinctly informed of the additions, if any, which he made to the text, while there appears considerable reason to suppose, that Lalande sometimes interfered with it. Certain it is, that many things would have been suppressed, and others added, had Montucla survived to finish the work of correcting and publishing. There is no reason to think that the eminent analyst referred to (Lacroix), would have supplied Montucla's omissions regarding the Poristic case in the Principia, or regarding the writers on the ancient analysis; for on this subject he was much better informed, in all probability, than Lacroix, and the

omission in the Principia comes less within the scope of modern than ancient geometry.

In the following letter, Mr. Carrick Moore has some doubt whether Mrs. M. was daughter of a brother or nephew of Dr. Simson. I have always heard my brother-in-law, the Admiral (Sir G. Moore) state, that his mother was the professor's niece:—

"PUTNEY VILLA, Sept. 28th, 1854.

"DEAR LORD BROUGHAM,

"Some time ago you wrote to inquire from me if I could recollect any particulars of Robert Simson, the celebrated geometrician. My mother's name was Jane Simson, niece or great-niece of Robert Simson, noted for being a very absent man. One day seeing her, he said, 'How do you do, Miss Fraser?' misnaming his relative, which proceeded from his knowing that the Simsons were originally Frasers, being of that clan. It was known, that the room in which Robert Simson lived was a den of confusion, he suffering no one to enter it to put it in order. My mother and another young lady had a violent curiosity to see this apartment, which he kept locked, and never opened but on hearing two Latin words pronounced: the ladies learned these words, and knocked at his door in the College—'Who's there?' was cried from the interior; the Latin words were spoken, and the door was slowly opened, when my mother's companion put in her arm, to prevent the professor shutting them out;—so they got in, he being amused with the stratagem. They saw the room filled with books, papers, mathematical and carpenter's tools, in inextricable confusion; but the professor got them at last a couple of chairs, from which he swept off the dust, and they sat down and chatted with him. The visit ended gaily.

"Another anecdote I recollect.—After a dinner given by one of the professors, amongst whom Robert Simson was a guest, the ladies and gentlemen went to walk in the College garden. It happened that a pigeon pursued by a hawk flew into Mr. Simson's bosom, who caught it; a gentleman seeing this cried, 'Throw the bird to the hawk,' jestingly,

on which Mr. Simson struck him to the ground by a blow of his fist, for he knew no use of language but to speak truth. But when his error was explained to him, he earnestly solicited pardon of the gentleman, and peace was made between them.

"I can recollect nothing more from my mother, and am now in the 92d year of my age, but still able to sign this.

"I am, my Lord, your humble servant,

"James C. Moore."

December 28, 1854.—"You may add to my former letter the following anecdote, which was related to my son by my sister. My grandfather, the Rev. Charles Moore, was minister of Stirling, and resided in the Castle, and received the celebrated geometrical professor on a visit; and after dinner the whole inhabitants of the Castle flocked round to see the professor mount his horse, who thanked them all for their kind leave-taking, and wished them all a pleasant journey back to Glasgow.

"I have the honour to remain, truly yours,

"James C. Moore."

The indistinct, if not inaccurate details, in Traill's Life, had occasioned the mistake, now corrected, of confounding the Dr. Williamson who in 1758 was appointed assistant, and in 1761 successor to the professor, with his favourite pupil, Dr. Williamson, afterwards chaplain at Lisbon. This was pointed out in a Note to the Caldwell papers, by Col. Mure, to whose learned and enlightened labours the cultivators of Greek literature are under such obligations. The fact of the professor's successor having held the office till 1796, well justified the supposition that he must have been a different person from the chaplain, who died young; and Col. M. suggested (vol. I., pt. 2, p. xv.) the possibility of there having been two persons of the same name, which upon inquiry turns out to be the fact. The Caldwell papers are one of the most important contributions to History, and particularly to Literary History, that have ever been made; and I owe to the kindness of the accomplished editor of this unpublished collection, the liberty of adding one or

two particulars regarding both Simson and Hume, from those papers, which he had presented to the Maitland Club of Glasgow.

It appears that a difference arose as to the terms on which the resignation in favour of Williamson had been made, and the matter was left to the arbitration of Matthew Stewart on Simson's part, and Baron Mure (the colonel's grandfather) on Williamson's. The correspondence throughout shows that there was no great favour entertained for him by the professor; but it also shows that his anxiety respecting the sum to be secured arose from the efforts he was making to pay off what remained of his father's debts, which as late as 1763 he had been unable to accomplish. We find also from those letters, that the number of his pupils had never been less than fifty, and often considerably more. The arrangement as to assistant and successor was universally practised in the Scotch Universities, but has of late been very properly discontinued, as leading to great abuse.

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